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PHYSIOLOGICAL AND ANATOMICAL.

VOL. II.

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RESEARCHES,
PHYSIOLOGICAL AND ANATOMICAL.

BY
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CONTENTS OF VOL. II.

I.	Page
An Account of some Experiments and Observations on the Blood	1
II.	
Miscellaneous Observations on the Blood	64
III.	
An Account of some Experiments on the Blood, chiefly in connexion with the Theory of Respiration	135
IV.	
Observations on the Blood after Death	109
V.	
An Account of some Experiments to endeavour to determine whether or no, free Carbonic Acid exists in the Urine of Man in Health	214
VI.	
Notice of some additional Experiments with the Air-pump, on some of the other Secretions, and on some of the Solid Parts of the Body	221
VII.	
On the Age of Morbid Adhesions, and of the Fluid found in the Pericardium after Death	237
VIII.	
On the Specific Gravity of different parts of the Human Body	253
IX.	
On the Effects of Desiccation, and the proportion of Water contained in the different Textures of the Human Body	269

	Page
X.	
On the Action of Corrosive Sublimate on the Textures of the Human Body	279
XI.	
On the Action of Lime on the Textures of the Human Body	290
XII.	
On the Action of Lime on Vegetable Substances	299
XIII.	
On the Effects of Tannin on the different Textures of the Human Body	305
XIV.	
On the Effects of Boiling Water, and of Boiling, on the Texture of the Human Body after Death	313
XV.	
On the Agency of Atmospheric Air on Dead Animal Matter, and on the Putrefactive Process	331
XVI.	
Experiments and Observations on the Maceration in Water of different Textures of the Human Body	372
XVII.	
General Remarks on Bichat's Theory of Membranes in connexion with the preceding Observations	413
XVIII.	
Observations on the Structure of the Ductus Communis Choledochus, and on the Flow of the Bile	427
XIX.	
On the Male Organs of some of the Cartilaginous Fishes	436
XX.	
Observations on Pus	462

EXPLANATION OF PLATES.

VOL. II.

PLATE I.

THE ductus communis choledochus laid open, (the gall-bladder inverted) shewing the peculiarity of structure, described page 427. In the instance, from which the drawing was made, the valvular structure towards the termination of the canal was rather more strongly developed than usual. The place of junction of the pancreatic duct is indicated by the lower probe; the upper probe is inserted under the transverse, somewhat spiral bands of the cystic duct.

a a portion of inner coat of duodenum.

b a portion of pancreas.

c a portion of the hepatic duct.

Fig. 2.—Testis, epididymis and vas deferens of torpedo.

a the testis.

b the epididymis.

c the vasa efferentia, connecting the two former.

d the vas deferens laid open, shewing its peculiar transverse lamellar structure.

PLATE II.

Testis and epididymis of *Raia clavata*.

a the globular testis.

b the milt-like or medullary portion.

- c* the epididymis.
d the tubuli connecting the testis and epididymis.
e portion of vas deferens.

PLATE III.

- Fig. 1.—Anal appendage of *Raia batis*, shewing the gland which belongs to it, in its natural situation, exposed by dissection and laid open.
- Fig. 2.—The same gland, shewing the peculiar cellular and contractile structure at *a*, conjectured to perform the part of an auxiliary heart.
- Fig. 3, 4, 5, 6.—Appearances of pus-globules under the microscope referred to page 477; in the three first instances magnified about 350 diameters; in the last about 300.

ERRATA.

- Page 94, *omit* reference to Phil. Trans. at bottom of foot note, and *insert* it at page 93.
- 101, line 6, foot note, *for* culicum, *read* cubicum.
- 185, „ 11, *omit* apostrophe after months.
- 202, „ 6, *for* connections, *read* concretions.
- 292, „ 6, *for* prostrate, *read* prostate.
- „ 7, *for* seminalis, *read* seminales, and *for* jejenum, *read* jejunum.
- 413, „ 16, *for* five, *read* fine.
- 429, „ 4, *before* had, *insert* have.

PHYSIOLOGICAL AND ANATOMICAL RESEARCHES.

I.

AN ACCOUNT OF SOME EXPERIMENTS AND OBSER- VATIONS ON THE BLOOD.

MANY of the following observations and experiments were contained in my inaugural dissertation on the blood, under the title of *Tentamen experimentale quædam de Sanguine conplectens*, written on the occasion of graduating, in 1814.

The views taken of the composition of the blood which I then adopted, and have not yet seen any reason, founded on facts, for relinquishing, were at that time, I believe I may say, commonly taught in the schools, at least in Edinburgh, and received as well-substantiated doctrines, resting chiefly on the researches of the great Physiologists of our own country, and especially of Hewson.

In making this remark, of course I allude to the general and broad views of the subject; not to details and special views deducible from particular

experiments, to which, considering the nature of the blood; and the accumulated experience of ages relative to the investigation of its properties, there seems to be hardly any limit : at least, we cannot at present discern the limit.

One of the distinguished physiologists of the 17th century, Lower, in the dedication of his very original work, *De corde, item de Motu et Colore Sanguinis*, appears to have thought it necessary to offer some apology to his friend for writing on such a topic : “Mirabuntur alii, forte et ipse miraris, vir ornatissime, de corde et sanguine post viros celebres, qui materiam hanc non tantum tractasse sed et exhausisse videantur, a me quicquam amplius proferri.” How different is the feeling now ; instead of exhausted, the inquiry seems, humanly speaking, rather inexhaustible.

1.—*Observations to endeavour to determine whether any heat is evolved by the blood in coagulating.*

Although the remarkable property of coagulation which the blood possesses has been laboriously and carefully investigated, yet, it must be confessed, we know but little respecting it ; it is still one of the many mysteries of nature. The little we know on the subject, appears to be chiefly this,—that the phenomenon depends on a portion of the blood, its fibrin, from being liquid, becoming solid, and from being generally diffused in uniform mixture, approxi-

mating and collecting in a mass, entangling the red particles and including a portion of the serum.

Now, as generally, during the conversion of liquids into solids, the mere change of form is connected with a disengagement of heat, it might, perhaps, *a priori*, be expected, that a disengagement of heat would be witnessed during the occurrence of the phenomenon in question.

Fourcroy has stated, that the effect of the blood's coagulation is attended with an elevation of several degrees of the thermometer.* Mr. Hunter, on the contrary, from his own experiments inferred that no heat is evolved on the occasion. The experiments, from which he deduced his conclusion, "that in the coagulation of the blood no heat is formed," were made on the blood of a comparatively cold-blooded animal, the turtle.† Notwithstanding the high authority of the inquirer, and the very favourable circumstances under which his observations were made, his experiments have not been considered entirely conclusive. Dr. Thomson and the late Dr. Gordon, from their own observations, were averse from his opinion, and rather favoured that of Fourcroy,—induced to do so, as stated, by Dr. Gordon, in his Lectures on Physiology, from witnessing the process of cooling of the blood either retarded or quite interrupted at the time of coagulation.

The consideration of these discrepancies induced

* Ann. de Chimie, vii. p. 147.

† Treatise on the Blood, &c. p. 28.

me to enter on the inquiry ; and I shall now describe some of the experiments made in carrying it on.

The subject of the first trials was the mixed venous and arterial blood of lambs. It was received from the divided vessels into a thin glass bottle of the capacity of eight and a half ounces of water. The bottle was quickly filled, and when full, closed with a perforated cork holding the projecting stem of a delicate thermometer, the bulb of which reached the middle of the vial.

Experiment 1.

The temperature of the blood the instant it

was drawn	-	-	-	104°
After 1 minute	}	coagulation beginning	-	103·5
„ 2 „			-	102·5
„ 3 „			-	102·5
„ 4 „	-	-	-	102·5
„ 5 „	-	-	-	102°
„ 6 „	-	-	-	101·75
„ 7 „	-	-	-	101·5
„ 8 „	-	-	-	101°
„ 9 „	-	-	-	100·5
„ 10 „	-	-	-	100°
„ 15 „	-	-	-	97·5
„ 20 „	-	-	-	95·0

This experiment was made in the open air in fine weather in summer, in the neighbourhood of Edinburgh, when the thermometer in the shade was 67°.

The following experiment was made at the same time: it differed principally from the preceding in two circumstances, viz. in the vial being left open, and in the bulb of the thermometer being near the surface of the fluid.

Experiment 2.

Temperature of blood when drawn	-	103°
After 1 minute	-	103°
2 } coagulation commencing	-	103°
3 }		
4 minutes	-	103°
5 "	-	102.75
6 "	-	102.5
7 "	-	102°
12 "	-	99°
17 "	-	96.5

At first view, the temporary pause in the cooling of the blood in these two experiments just after the commencement of coagulation, would seem to indicate an evolution of heat, commensurate with the arrested cooling effect: but, as the result may be owing to other causes, this conclusion, however specious, may be erroneous. It appeared to me therefore advisable, to repeat the above experiments on cooling, as accurately as possible, substituting water for the blood. I thus reasoned; if the results in regard to the rate of cooling of the blood and water should be similar, the thermometer being stationary for

a time, must depend on some other cause than coagulation; on the contrary, if the fall of the thermometer in the cooling water should be regular and uninterrupted, the pause in the instance of the blood might fairly be attributed to and be accounted for by a certain degree of heat evolved.

Experiment 1.

Temperature of water on admission	-	102°
After 1 minute	-	102°
„ 2 „	-	102°
„ 3 „	-	101.5
„ 4 „	-	101°
„ 9 „	-	99°

Experiment 2.

Temperature of water	-	106.5
After 1 minute	-	106.5
„ 2 „	-	106.25
„ 3 „	-	105.75
„ 4 „	-	105.25
„ 5 „	-	104.75

Experiment 3.

The bulb of the thermometer immersed in the warm water in the middle of the bottle being at

100°, was suddenly thrust to the bottom; it immediately rose to 102°.

These comparative results are manifestly not in favour of any heat being evolved during the coagulation of the blood; from them, it appears most probable, that the pause in the descent of the thermometer is rather owing to a disengagement of heat, from the warmer bottom of the vessel and the deeper fluid.

With the hope of removing all doubt on the point at issue, the following experiment was instituted. To guard against rapid cooling, chiefly the effect of exposure to the air, the bottle for receiving the blood was enveloped in a large quantity of fine wool, which is one of the worst conductors of heat; a thermometer, as before, was introduced into the middle, through a cork, and the vessel was filled with warm water of the temperature 108°. Thus prepared, the carotid artery of a lamb was divided; the bottle was emptied of water, and as soon as possible filled with arterial blood. The thermometer immediately stood at 104·5; it was thus stationary during five minutes; and even after twelve minutes it had fallen only half a degree.

From these results generally, it appears to me, the conclusion is unavoidable, that little or no heat is evolved from the blood, as the effect of coagulation. If it be asked, is this an exception to the general rule, before announced, I would answer, that it is not. It was before observed, that what is

commonly called the coagulation of the blood, is merely the coagulation of one of the ingredients of this fluid, viz. the fibrin, which constitutes only a very small part of the whole mass; for instance, in the blood of the lamb, it does not exceed 2 per cent. as I have ascertained by experiment. Moreover, it is worthy of remark, that though the coagulation commences suddenly, and is apparently rapid, yet the contraction or condensation depending on the coalescing of the solid particles, is slow; requiring many hours for its completion. All these circumstances considered, even admitting the law referred to, how small should be the degree of heat evolved in accordance with it, during the act of coagulation! So small indeed, that it can hardly be admitted to be cognisable by our senses; its detection, I must confess, would appear to me almost miraculous.

In confirmation of this conclusion, I shall give some additional facts on the subject, which I had an opportunity of collecting in 1816, on my voyage to Ceylon, in experimenting on the blood of the turtle and shark.

On the 15th of March, when our ship was in latitude $4^{\circ} 9' N.$ and longitude $19^{\circ} 15' W.$ by chronometer, at sunset, a large shark was taken by means of a harpoon. As soon as it was brought on board, while it was yet alive, it was cut in two. The blood flowing from the great dorsal vein was 82° ; the surrounding thick muscles were 82.5 , the water of the sea was 80.5 , and the air 79° . Some of the blood

was collected in a glass vessel. In about two minutes *it* had firmly coagulated. During the whole time I watched the thermometer immersed in it. The mercury sunk from $81\cdot5$ to 81° ; it did not rise at the instant that the coagulation commenced, nor did it remain stationary whilst the coagulation was going on, but continued gradually sinking.

The day following another shark was taken. The same experiment was made with the blood, and a similar result was obtained.

On the 23rd of March, when in latitude $2^{\circ} 29'$ S. and in longitude $24^{\circ} 30'$ W., a large turtle was killed, which had been caught about three weeks before at the island of Ascension. The air at the time was 79° ; the blood of the turtle, flowing from the carotids, was 91° . When collected in a tumbler it was $88\cdot5$. The thermometer placed in the midst of it, immediately began to fall, and continued falling gradually, without any sensible interruption, whilst the blood was coagulating.*

* On this subject, I made, in Ceylon and in Malta, some additional experiments. Bearing date of Colombo, May 4th, 1817, is the following note, which I shall transcribe verbatim. "Testudo Mydas—about 50 lbs. weight—caught yesterday; thermometer in recto $84\cdot5$; stream of arterial blood flowing from the divided carotids 85° ; temperature of air 86° .

"Made three experiments with the blood to ascertain under such favourable circumstances, whether any heat is evolved during the coagulation of the blood. The general results proved decidedly that there was no sensible heat evolved.

"The blood, as it flowed, was 85° ; collected in a glass vessel it was the same; it coagulated in about a minute,—still no change; in

These results were inserted in a paper published in the second volume of the Journal of the Royal Institution, with some others, in reply to Dr. Gordon. This gentleman, about eighteen months previously, had endeavoured to support his original opinion by further experiments, an account of which appeared in Dr. Thompson's Annals of Philosophy. I shall add my remarks on them, believing that they may help to explain the effect, in accordance with what I witnessed.

The difference between the results of Dr. Gordon's experiments and my own arose, perhaps, from the different modes in which our experiments were made. Dr. Gordon, I believe, kept the bulb of his

about two minutes the coagulum was firm,—temperature still the same. In about half an hour the blood had acquired the temperature of the air, which was then 87° ."

And another, bearing date of the 23rd October, 1819.—"A large green turtle was brought to me this morning. The great vessels of the neck were immediately divided. A wine glass was quickly filled with arterial blood: a thermometer in it rose to 82° (air at the time 79°); in two minutes it was coagulated slightly—thermometer still 82° ; in five minutes it had coagulated firmly—thermometer 81° . A very delicate thermometer was used."

At Malta, a similar trial was made on the 21st December, 1838, on the blood of a turtle, which had been kept about two months in water, without eating, and yet was active. The temperature of the water was 58° , that of the air of the room 60° , two hours earlier it was 58° ; the blood of the turtle, flowing from the great vessels of the neck (the jugular veins and carotid arteries) was 58° . It was collected in cups, and, in about half an hour, was firmly coagulated. During this time, whilst the coagulation was taking place, I tried, repeatedly, the temperature of the different portions, with a delicate thermometer, but could perceive no change.

thermometer near the bottom of the vessel containing the blood,—and when this fluid began to coagulate towards the surface, he drew the instrument up. On the contrary, in all my latter experiments, viz., those on the blood of the shark and turtle, and many which were made on the blood of sheep at the Cape of Good Hope, the thermometer was not allowed to remain stationary,—it was gently moved from one part to another, so that the whole might be kept of the same temperature, till the coagulation commenced;—for when the blood is viscid and the vessel deep, the surface or the portion last drawn is warmer than that below,—and when shallow, the bottom is warmest, as I have frequently observed in my experiments.

Further, in confirmation of the conclusion that no appreciable degree of heat is noticed during the coagulation of blood,—I may mention, analogically, the fact, that when the serum of the blood is coagulated, by means of dilute nitric acid, there is not the slightest elevation or change of temperature. The following are the particulars of an experiment in proof of this. An ounce of serum was added to an ounce of dilute nitric acid, each of the temperature of the air of the room, viz., 75° ; coagulation was immediately produced, but not the slightest change of temperature was occasioned; the mixture was exactly 75° . The dilute nitric acid used contained five parts of water to one of strong acid. The serum employed was of specific gravity 1.030; and

when slowly evaporated to dryness, an ounce of it afforded a brittle translucent residue, weighing 36·7 grains. It was procured from the blood of a patient labouring under a tedious chronic disease; which blood was only of specific gravity 1·047; and an ounce of it afforded 2·4 grains of dry fibrin.

The inference from this fact, reasoning analogically, seems to me unavoidable, viz., that it is highly improbable any heat can be given off, during the coagulation of the blood, in which so small a quantity of matter becomes solid, and that slowly,—since not the smallest degree is evolved during the coagulation of serum, when so large a quantity of matter, so similar to fibrin in its nature, is solidified, and that suddenly.

I would remark, in conclusion, that as regards the general rule before referred to, of change of temperature with change of form,—it has so many exceptions, that no reliance can be placed on it. Every instance requires to be specially considered and determined experimentally. Thus, contrary to rule, theoretically considered, all the detonating compounds, whether they contain oxygen or not, emit heat in the act of explosion, and of conversion entirely, or in part, into the state of an elastic fluid; thus, carbonic acid when expelled from lime by dilute muriatic acid, is converted from the solid to the gaseous state, without any change of temperature; and thus, further, when ammoniacal gas and muriatic acid

gas combine they form a solid salt, without the slightest disengagement of heat.

2.—Observations to determine whether there is any well marked difference, in point of time, in the coagulation of venous and arterial blood, and in the degree of contraction accompanying it.

To endeavour to determine the first question, the following experiments were made, in Edinburgh, during the summer of 1811, on the blood of lambs. The two kinds of blood compared were taken from the same animal,—one from the jugular vein, the other from the carotid artery; and each was received in a glass vessel, either the same, or similar, of about three-ounce capacity. The thermometer in the open air, where the experiments were made, stood at 70°.

1. Venous blood;—in four minutes there was a commencement of coagulation; in six it was firmly coagulated.

Arterial blood;—in half a minute it had begun to coagulate; in two minutes it was firmly coagulated; and, in three, there was a distinct separation of serum.

2. Venous blood;—in two minutes there was a commencement of coagulation, and, in two and a-half, it was firmly coagulated.

Arterial blood;—its coagulation commenced as it

flowed from the vessel; in a minute it was firmly coagulated.

3. Venous blood;—in two minutes it was slightly coagulated.

Arterial blood;—in one minute it was firmly coagulated.

From these experiments and others, the results of which were similar, and which, therefore, it is unnecessary to describe, it appears that arterial blood coagulates with greater rapidity than venous.

Relative to the degree of contraction attending the coagulation of each kind of blood,—it was unquestionably greatest in the instance of arterial; the difference of appearance was so distinct, measured merely by the eye, that there could be no mistake. In every comparative trial, after many hours, the venous blood was found softer and less contracted.

3.—*Observations on the degree of coagulability of the first and last portion of blood from a slaughtered animal.*

Mr. Hewson observed that blood which flows last from a slaughtered animal coagulates more readily than that which flows first.* Mr. Hay, in his work on the blood, inferred the contrary from his own experiments.† The trials which I have instituted on this point, using the blood of the lamb and sheep,

* Hewson on the Blood, p. 62.

† Hay on the Blood, p. 26.

perfectly accord with those of Hewson. I shall give a few of the results. The same glass vessels were employed to collect the blood in as before. The temperature of the air was 68° . In one vessel, the blood which first gushed out, on the division of the great cervical vessels, was caught,—and, in another similar, that which flowed just before the animal expired.

1. On the sheep.—The blood which flowed first, was firmly coagulated in two minutes ; but that which flowed last, in half a minute.

2, 3 and 4.—In three experiments on lambs, the results were perfectly similar ; the first portion coagulated in one minute ; the last in half a minute.

It is not easy to reconcile these results, and those of Hewson, with Mr. Hay's. His perhaps may be held as exceptions. The two former at least shew that in certain cases, the blood which flows from an exhausted animal possesses an unusual proclivity to coagulate : and if consequences are attended to, it is difficult to avoid considering it, whatever may be its cause, as a happy provision of nature to aid in arresting hemorrhage.

4.—*Observations on the specific gravity of the different parts of the blood,—viz. the serum, coagulable lymph, and red particles.*

Mr. Hunter, from a few very simple experiments, arrived at the conclusion, that of the different con-

stituent parts of the blood, or those in which it is resolvable when abstracted from the vessels, the red particles are of greatest specific gravity, the serum of least, and the coagulable lymph intermediate. That the red particles are as he considers them, is now universally admitted : but it has been more than doubted by one inquirer, that the serum is lighter than the lymph ;* and by another, that there is any difference of specific gravity between them.†

I have repeated the experiments, on the results of which Mr. Hunter founded his conclusion, and I have no hesitation in adopting it entirely. In every instance in which according to his method, I have immersed coagulable lymph in serum, I have invariably seen it sink, unless buoyed up by adhering particles of air.

The specific gravity of the different parts of the blood is probably variable within certain limits, as is the specific gravity of the blood itself even within the limits of health, according to a variety of circumstances, which it may be difficult to enumerate, and much more to appreciate, connected with age, sex, climate, diet, &c.

I shall not in this place endeavour to determine what may be considered the standard specific gravity either of the blood itself, or of its serous or fibrinous parts. To accomplish this, a very wide comparison of accurate observations would be re-

* Sir C. Scudamore. Essay on the Blood, p. 35.

† Dr. Benjamin Babington, Med. Ch. Trans. of London, vol. xvi. Part II. p. 304.

quired ; beyond my reach ; and which I fear, in the present state of our knowledge, is hardly practicable.

From the results of the experiments which I have had an opportunity of making on healthy serum, and which will be individually given hereafter, I may state that I have found it to vary between 1020, and 1031.

The trials which I have made of the specific gravity of coagulable lymph or fibrin have been comparatively few in number. I can find mention of three only in my note-books. In the 1st, it was found of specific gravity 1046 ; in the 2nd of 1057 ; and in the 3rd, 1060. The first was procured from blood not buffed ; was separated by washing with water, and the fibrin or lymph thus obtained, was weighed after having been gently pressed between folds of blotting paper. The second, was a fibrinous concretion from the heart, free from colouring matter ; it was weighed without having being subjected to the action of water, after having been gently pressed in blotting paper. The third was a portion of buffy coat ; it was weighed after having been well washed with water, and the excess of moisture removed, as in the two preceding instances, by paper.

My experiments on the specific gravity of the red particles have also been few in number. From those on the results of which I think most reliance can be placed, it would appear, that the specific gravity of a red particle is about 1132, which

very nearly agrees with the estimate of Jurin, indirectly obtained, made more than a century ago.* The method by which I arrived at this result, was by putting a minute portion of blood, less than a drop, in solutions of different degrees of strength of sulphate of soda. In one of specific gravity 1090, the red particles slowly subsided: in that of the specific gravity above mentioned, when agitated in it, they remained for some time suspended, but rather inclining downwards than upwards. This was nearly a saturated solution at a temperature of about 120° : it ultimately deposited a considerable quantity of crystals; but it remained without change sufficiently long to allow of the trial described being made. This result is perhaps more deserving of confidence in as much as the red particles did not appear to be materially changed by the strong saline solution; under the microscope they exhibited their usual disc-like form.

Relative to the cruor, or admixture of the red particles and serum, I may mention one or two trials which I have made on its specific gravity, weighing it in the usual manner. A mass of crassamentum from blood the specific gravity of the whole of which was 1055, and the serum of which was 1029, was broken up and subjected to moderate pressure in a linen bag without any addition of water. The liquid which passed through, consisting of red particles and a little serum, was found to be of specific gravity 1074. This

* He estimated it at 1126. Phil. Trans. for 1719.

cruor was next poured on a filter of cotton cloth, to drain off as much as possible of the serum. It was left for twenty-four hours covered over with a glass receiver to prevent loss by evaporation. Now weighed again it was found of the increased specific gravity 1087. It was of the consistence of melasses; and was not further concentrated by being kept in a filter of very bibulous paper: the paper became saturated with it; but not a single drop separated.

Probably the phenomena of the buffy coat have given rise to the idea, before alluded to, in opposition to Hunter, of the comparative lightness of fibrin. The buffy coat appears on the surface of the blood, and therefore seems naturally to suggest the idea. But when the circumstances are inquired into, under which it forms, there is no difficulty in relinquishing it.

In sily blood, or that blood which affords a buffy coat, the serum and coagulable lymph appear to be of greater tenuity or less viscid than in blood that is not sily; and sily blood is generally slower in coagulating than blood that is not sily, although not universally so. When the former quality alone prevails, the buffy coat which forms is thin; when both qualities coexist, the buffy coat is thick, from the greater depth of the subsidence of the heavier red particles. Now, though the fibrin of the buffy coat is of greater specific gravity than the serum,—the *liquid* mixture of coagulable lymph and of serum,—the liquor sanguinis, as it has recently been called,—is of less spe-

cific gravity than the liquid mixture of red particles, serum, and coagulable lymph, and in consequence the former becomes supernatant. Before coagulation takes place, it is this comparatively dense fluid which supports the liquid fibrin, and when coagulation has occurred the substratum of red particles enveloped in fibrin still gives support; and, the longer the coagulation is in taking place, so much the less fibrin is mixed with the red particles, so much the more rises or is pressed up, and, *cæteris paribus*, so much the thicker the buffy coat becomes. In healthy blood, on the contrary, in which the serum and coagulable lymph are more viscid, the red particles are supported till coagulation takes place, and then are retained, like water in a sponge, and, like water, may be expressed from the sponge-like texture; or, if in excess, a part of them will separate and fall down as the fibrin contracts. This view is in accordance with Hunter's well authenticated facts, confirmed as above. And, as another experimental confirmation of them, I may mention, that when fibrin, or a portion of buffy coat which sinks in serum, is plunged into the cruor of blood, it rises to the surface and floats, in accordance with their respective specific gravities.

Relative to the second opinion, already referred to, that there is no difference of specific gravity between serum and lymph, it appears to me inadmissible, simply because it is not in accordance with facts. The ingenious author of it seems to found the idea

chiefly on the circumstance of the uniformity of composition of the crassamentum in relation to fibrin. Granted that the crassamentum is uniform in this respect, that there is no accumulation of fibrin towards its bottom, and no deficiency of it towards the surface, it does not therefore follow that the fibrinous part, not even when liquid, as coagulable lymph, is of the same specific gravity as the serum with which it is equally mixed, and much less when solid. It might, it appears to me, as well be said, that the serum of the blood and water is each of the same specific gravity, because they are miscible, and that equally. The mere circumstance of the coagulable lymph and serum being in intimate mixture, appears to me to be quite sufficient to account for the fact of the uniform composition of the crassamentum, admitting it to be so.

Dr. Benjamin Babington has connected the above idea with some views peculiar to himself, relative to the liquid part of the blood, which I must confess do not appear to me to be well borne out by the facts. The opinion of Hewson and of Hunter appears to me more in harmony with them. They considered the fluid part of the blood an equable mixture of liquid lymph, and of serum; both capable of being separated from the mass of the blood, apart, or together, under peculiar circumstances in health and disease; the one spontaneously coagulable, the other not. Dr. Benjamin Babington, on the contrary, is of opinion, that no such fluid as serum

exists in the blood and no such fluid as coagulable lymph; but a liquor sanguinis, not capable, compatibly with health, of affording either serum alone, or lymph alone, and capable of being separated into serum and lymph or fibrin, only when removed from the circulation, and not under the influence of the laws of life. And in accordance with this view he considers the denomination of serous membrane improper.

If serum were never found in closed cavities, without its due proportion of lymph (taking as a measure the albuminous part of each) then this view would be plausible; but as this is not the case—as serum, differing very little in specific gravity from the serum of the blood, is not unfrequently to be found in such cavities, especially in the pericardium, unaccompanied by any lymph (as I have often satisfied myself) or by the deposition of lymph in the form of a false membrane, the hypothesis appears to me untenable. Dr. Babington admits the facts I have alluded to in the latter part of his paper, where, contrary to what he advances in the first part of it, he has recourse, to explain them, to the ordinary doctrine of secretion.

5.—*Observations on the Specific Gravity of Blood, Venous and Arterial, of different Animals, and of different Ages, not including Man.*

As a preliminary, it may be right to mention the

method employed in the trials, the results of which are about to be given. It was the same as that now commonly used for ascertaining the specific gravity of other fluids. On entering upon the inquiry, I provided myself with a certain number of thin globular bottles, varying in capacity from 200 to 400 grains of water. Each bottle had a narrow neck, to which was adapted by grinding a perforated glass-stopper, so as to admit of accuracy in filling. The weight of the fluid examined was always compared with that of distilled water; and trial of the latter was made as often as any material change of temperature occurred to render it advisable. In each instance, before making trial of the blood, it was allowed to acquire the temperature of the atmosphere, and then the small vacant space in the bottle, the effect of cooling, was filled with serum. In the earliest experiments a balance was used of no great delicacy: it was affected, however, readily by the one-tenth of a grain. In the latter, one was substituted which was affected by the one-fiftieth of a grain; and in the latest of all, one still more delicate, constructed by Robinson, which when loaded with 600 grains, was turned by an hundredth of a grain.

For the sake of brevity, I shall give the results of my experiments in a tabular form.

TABLE I.

Shewing the specific gravity of blood, venous and arterial, of different animals, and of different ages, not including man.

Animal.	Age.	SPECIFIC GRAVITY.					Place.	Season.
		Arterial blood.	Venous blood.	Arterial serum.	Venous serum.	Mixed venous and arterial blood.		
Sheep	1	—	1050	1056	1025	1027	Edinburgh.	Summer.
"	2	6 years.	1057	1058	1030	1030	"	"
"	3	16 mths.	1049	1051	—	—	"	"
"	4	"	1047	1050	—	—	"	"
"	5	3 years.	1047	1051	1019	1029*	Malta,	"
Lamb	1	11 wks.	1052	1055	1027	1028	Edinburgh.	"
"	2	"	1046	1057	1024	1024	"	"
"	3	"	1054	1054	1024	1024	"	"
"	4	"	1050	1053	1024	1024	"	"
"	5	"	1047	1050	—	—	"	"
Horse	-	12 years.	—	1053	—	1027	Chatham.	Spring.
Zebra	-	—	—	1053	—	—	Edinburgh.	Autumn.
Ox	-	—	1058	1061	1027	1029	"	Summer.
Calf	-	—	1040	1046	1022	1023	"	"
Dog	-	—	1048	1053	1022	1023	"	"
Pig	-	—	—	1060	—	—	"	"
Cat	-	—	—	1050	—	—	Chatham.	Winter.
Turkey	-	—	1061	—	1021	—	"	"
Salmon	-	—	—	—	—	1051	Edinburgh.	Summer.
Holybut	-	—	—	—	—	1028	Chatham.	Spring.
Cod	-	—	—	—	—	1034	"	"
Plaice	-	—	—	—	—	1032	"	"
Skate (R. batis)	-	—	—	—	—	1035	"	"
Dogfish (M. lævis)	-	—	—	—	—	1022	"	"
Eel (M. latirostris)	-	—	—	—	—	1042	"	"
Frogs, males	-	—	—	—	—	1040	Edinburgh.	Summer.

* In this trial, the venous blood was first received; the arterial when the vessels were nearly emptied.

In all the instances of the mammalia the blood taken was from the jugular vein and carotid artery.

All the animals were in apparent good health at the time the trials on the blood were made, with the exception of the zebra, which had a tumour in the nostril, and of which it died after a few weeks.

From the results stated in the table, it clearly appears that arterial blood is of somewhat less specific gravity than venous, and arterial serum than venous serum. Taking the mean of all the experiments, the specific gravity of each kind of blood and serum, appears to be as follows :—

Arterial blood	1050
„ serum	1022
Venous blood	1053
„ serum	1026

Relative to the cause of the greater specific gravity of venous blood, it probably does not solely depend on the arterial blood's containing a larger proportion of water ; it probably depends partly on this cause and partly on the venous blood containing, besides a smaller proportion of water, a larger proportion of animal matter. But what the kind of matter is, whether the coloured particles, or fibrin, or some other substance, as free carbon, remains to be determined. Independent of this point, there seems to be little difficulty in accounting for the difference, considering the many fluids which are secreted from the arterial blood, and those comparatively dilute, and thereby manifestly tending to augment the

density of the venous blood. It was long ago observed by Dr. Crawford, that arterial blood is apparently more liquid than venous ; if so, in reality, which is probable, it may be partly owing to its being more dilute, as indicated by its specific gravity.

The number of experiments on the specific gravity of the blood and serum of animals of different ages is hardly sufficiently large to allow of any satisfactory conclusion ; they seem to indicate that the blood of adult animals is of greater specific gravity than that of young—an inference in accordance with analogy, and which Bryan Robinson, one of the ablest of the iatro-mathematical sect, many years ago deduced from his own experiments.*

That the specific gravity of the blood of different animals varies, is manifest from the preceding table. The difficulty is to reply to the question, in what manner the variation accords with the nature or species of animals.

As the degree of density of the blood appears to depend chiefly on the proportion of red particles ; and as the use of the red particles seems to be connected less with nutrition than with action, according to the views of Hunter,† and more, I may add, with the production of animal heat, than perhaps with any other function, it is very probable, that the blood of birds is of greatest specific gravity, (varying in degree according to the temperature of each

* Vide Animal Economy, p. 245.

† Hunter on the Blood, p. 431.

individual, and perhaps its strength and energies) —that the specific gravity of the blood of the mammalia is next,—next that of reptiles,—then that of fishes,—and lastly, that of the different classes of animals having colourless blood, amongst which it is probable, grounded on the preceding considerations, that the blood of insects will hold the highest place. Many things already known favour this view of the subject: but still it must be considered hypothetical, until it has been fairly investigated experimentally.

6.—*Observations on the specific gravity of the blood of animals blooded to death.*

To endeavour to determine whether, in the instance of animals blooded to death, any difference exists in the specific gravity of the blood which flows first, and that which flows last,—I made a few experiments in Edinburgh in the summer of 1812, the results of which are contained in the following Table.

TABLE II.

ANIMAL.	SPECIFIC GRAVITY.			
	1st. Arterial Blood.	2nd. Arterial Blood.	1st. Serum.	2d. Serum.
Sheep 1.	1049	1048	1024	1023
„ 2	1050	1044	1027	1022
Lamb 1	1049	1046	1024	1020
„ 2	1051	1045	1024	1018
Ox 1	1058	1051	1027	1021

All these results seem clearly to shew that the blood which flows last, when the vessels of the animal are nearly exhausted of their fluid contents, contains a diminished quantity of animal matter, or an increased quantity of the aqueous part. And that the difference of specific gravity is mainly owing to the latter, rather than the former cause, may be inferred from the circumstance, that there is not a greater difference in the specific gravity of the blood before the separation of the serum, than in the serum itself. Whence it may be asked, is this dilution of the blood, this augmentation of its

watery part? Does it depend on increased activity of the absorbent vessels, in connexion with an undue languor of the terminal secerning arteries? or solely on the debility of the latter, the action of the former remaining neither diminished nor altered? It appears to me, that the latter supposition is most probable, and is most accordant with the general doctrines of pathology and therapeutics.* I know not whether the results obtained by Mr. Hay, will bear the same interpretation. He, from his experiments, inferred that the last portions of blood which flowed contain less serum than the first, in the ratio of 28·8 to 55·8.† But his results are defective, inasmuch as he did not determine the specific gravity of the serum, merely the quantity of it which separated in the act of coagulation. The crassamentum of the blood, as already observed, and as is now generally admitted, consists of red particles, serum and fibrin,—the serum contained or held in the interstices of the fibrin as in a sponge; and consequently, the softer the fibrinous mass is, or the less contracted, so much the more serum it must hold. These remarks apply to the deductions of Mr. Hay: the *argumentum ad absurdum* is also applicable to them; for

* It may be owing to an unusual flow of the contents of lymphatics into the circulation, as the blood flows out: the lymph certainly is of much less specific gravity than the blood,—whether spontaneously coagulable as Hewson found it, in different instances in which he examined it, or only coagulable by heat, as I have occasionally found it in the lymphatic vessels of the spermatic cord of the bull.

† Op. cit. p. 28.

by adding water to the blood before coagulation, the volume of the coagulum is increased, and the separation of fluid from it is diminished: therefore the inference from the quantity of serum, independent of its specific gravity, is of no weight in this question.

7.—*Observations on the specific gravity of human blood and serum in instances of disease.*

The following observations were made at three different periods, viz. in the years 1811-12, in Edinburgh; in 1818-19, at Kandy in the Island of Ceylon, and in 1826-27, in Corfu. As both the climate and the circumstances affecting the condition of the individuals were very different, I shall give the results obtained at each period, in a table apart.

In these instances the same precautions were taken in ascertaining the specific gravity of the blood as in the former: and, I may add, that the blood from which the serum was collected for trial, was kept so as to prevent evaporation, either in a bottle and corked, or defended by a cover if in a bleeding-cup. The necessity of this to insure accuracy is obvious: if the air have free access to the surface of the blood, a portion of the aqueous part will necessarily be carried off by evaporation,

and the specific gravity of the serum will be augmented.

1.—*Table shewing the specific gravity of blood and serum in instances of disease in Edinburgh.*

TABLE III.

Age and Sex.	DISEASE.	Sp. Gr. of Blood.	Sp. Gr. of Serum.
WOMEN.			
30 years	Phthisis pulmonalis: the blood not buffed: little pyrexia - - -	1054	1027
25 "	Mild pneumonia - - -		1023
24 "	Severe catarrh, with much pyrexia -	1061	1028
22 "	Pleuritis: the blood buffed - -	1058	1026
26 "	Severe headache: slight pain of side: slight pyrexia - - -	1056	1031
40 "	Hemiplegia - - -		1026
27 "	Emansio mensium - - -	10518	1028
Ditto	Convalescent - - -		1029
17 "	Chlorosis.—Serum green - - -	1055	1027
60 "	Slight cough and pain of side, without pyrexia - - -	1059	1026
MEN			
20 years	Severe headache, the effect of high temperature; without pyrexia - -	1062	1026
33 "	Severe headache during convalescence from fever - - -	1061	1027
23 "	Pneumonia: blood slightly buffed -	1060	1025
23 " } Negro }	Slight pneumonia - - -	1061	1033
19 years	Phthisis pulmonalis; a short time before death - - -	1045	
42 "	Diabetes mellitus - - -	1061	1030
19 "	Diabetes mellitus - - -		
	Blood drawn at evening - - -	1060	1025
	Blood drawn in the morning - - -	1062	1026
	Blood drawn at evening with milky serum - - -	1058	1026
	Blood drawn in the morning not milky -	1050	1023

The blood in each instance was venous, and was taken from the arm. The individuals from whom it was abstracted, were patients in the Royal Infirmary, and belonged chiefly to the working class, and were, with few exceptions, inhabitants of the town.

The results are too limited in number to admit of any general conclusions being deduced from them in a clear and satisfactory manner. The following inferences, however, they seem to warrant, as probable;—viz. that in inflammatory complaints the proportion of animal matter is increased, the proportion of water diminished; and also that the density of the female blood is less than that of man. The former inference accords with the experiments of Robinson, Tabor, and Langrish, which although imperfect in themselves, may at least perhaps be depended on for comparison. In the majority of the examples adduced, nothing unusual was manifest in the appearance of the blood. Whenever the blood exhibited a buffy coat, the inferior surface of the mass of crassamentum appeared softer than usual, as if from a deficiency of fibrin; which is not surprising, considering that the specific gravity of the red particles is greater than that of the fibrin. The green colour of the serum of the chlorotic woman is worthy of remark;—its cause, I was not able to ascertain: it did not appear to depend on the presence of bile. The blood of the man labouring under diabetes, was in each

instance less firmly coagulated than healthy blood. In two instances, the serum of this blood, last drawn, had a milky hue: a common occurrence in this disease, and which I first learned from the Lectures of Dr. Home. The serum of this blood I frequently examined according to Dr. Wollaston's method, but unsuccessfully; I could never detect in it a particle of saccharine matter: nevertheless, I believe it contained an undue proportion of animal matter: this was indicated by its comparatively high specific gravity. In the last case of diabetes, the depleting plan of treatment was fairly tried: blood was abstracted for some time every other day, and the effect in diminishing the specific gravity of the blood and the serum was well marked. In this case, further, it may be mentioned that some relation appeared between the blood and the urine. The specific gravity of the urine was always greatest in the morning; when it uniformly exceeded 1040, and was once as high as 1051; and yet in other respects, it then appeared most analogous to healthy urine; it abounded in urea, and contained hardly a trace of saccharine matter. In the evening, on the contrary, when its specific gravity was least, viz. between 1030 and 1040, the urea gave place to saccharine matter: the former could not be detected in it; the latter abounded. And, like the urine, the blood in this case was of greater specific gravity in the morning than in the evening.

2.—*Table shewing the specific gravity of blood and serum, and also of bile in certain instances of disease, in the interior of Ceylon.*

TABLE IV.

Age.	Disease.	Blood.	Serum.	Bile.
25	Fatal remittent fever; no organic disease could be detected by dissection; the liver appeared healthy; it weighed 4lb. 10oz.; about an ounce of glairy yellow bile in gall bladder.	—	—	1011
27	Convalescent from a severe attack of remittent fever; seized with pain of side.	1054	—	—
23	Fatal remittent fever; about 2oz. of brownish bile in the gall bladder; no well-marked lesion discovered.	—	—	10133
35	Remittent fever (2d day); blood not buffy; serum of the colour of ale.	10541	10238	—
	24 hours after, after having been well purged, was blooded again on account of severe headache.	—	10240	—
42	Slight remittent fever (3d day); blood not buffy; serum tinged slightly red.	10585	10281	—
30	Intermittent fever; 8oz. of blood abstracted in hot stage, which it cut short; blood of natural appearance; serum of reddish hue.	10588	10281	—
31	Fatal remittent fever, with yellowness of skin, and bloody urine; the inner coat of bladder of urine, of stomach, and duodenum, red, as if inflamed; liver soft; gall bladder turgid with dark, thick, ropy bile; mouth of common ducts apparently inflamed, but not obstructed.	—	—	10556
29	Continued fever (3d day); second V. S. not buffed; very dark and soft.	10551	10281	—
40	Remittent fever (2d day); it began as intermittent, on a severe march; pain in hypochondria; buffy coat of blood near half an inch thick; serum light yellow.	—	10290	—
40	Remittent fever with pain of chest (7th day) very slight buffy coat.	—	10250	—
35	Ephemeral fever, after confinement in Hospital for a peculiar debility of legs; no buffy coat; crassamentum soft; serum limpid and red.	—	10279	—
25	Fatal remittent fever; liver weighed 5lb. 4 oz.; bile dark and ropy.	—	—	10445
35	Continued fever (6th day); blood not buffed; serum very yellow.	—	1022	—
35	Slight pneumonia; blood not buffed; serum of reddish tinge.	1055	1027	—

TABLE continued.

Age.	Disease.	Blood.	Serum.	Bile.
35	Severe catarrh; some pain of chest; no buffy coat on blood; serum of reddish hue	—	10295	—
30	Negro:—Slight pneumonia; blood not buffed; serum of ale-colour	—	10296	—
	2d day; aggravation of complaint V. S. repeated; collected in two portions; first portion not buffed; of natural appearance	—	10296	—
	2d portion, a good deal buffed and cupped	—	10275	—
30	Sepoy:—Pneumonia; a week ill; blood of natural appearance, collected in two portions; serum of the colour of porter; first	—	10286	—
	second	—	10281	—
40	Epilepsy, with severe headache	—	10295	—
25	Hepatitis, with occasional dysenteric symptoms; 2d V. S. Blood collected in two portions; first portion slightly buffed; second, of natural appearance; both of	—	10274	—
27	Chronic hepatitis; blood not buffed; serum greenish yellow	—	10281	—
30	Hepatitis; blood not buffed; serum of the colour of ale, after a severe and harassing march	—	10316	—
30	Acute dysentery (10th day); 2d V. S. Blood very slightly buffed and cupped	—	10288	—
	3d V. S. after half an hour; blood even less altered	—	10278	—
30	Acute dysentery; blood not buffed; serum slightly tinged red; after a severe and harassing march.*	—	10303	—
30	Acute dysentery; blood not buffed; serum light greenish yellow	10565	10281	—
	Died on 6th day. Rectum and colon gangrenous. Bile of natural colour, ropy	—	—	10371
31	Acute dysentery, fatal on the 10th day; inflammation, thickening and ulceration of large			

* This man was one of a chosen party of about 60, who in 21 hours made a most severe march from Atgalé to Nalandi, and after sleeping on the ground two or three hours, returned in about the same time. They were sent to relieve a post which they found evacuated. Great part of the way, both in coming and going, they were opposed by the enemy, and found the road variously obstructed. Their exertions were extreme; every man of the party, with the exception of the commanding officer, was in the sick-list within the next fortnight, labouring under remittent fever, dysentery, and hepatitis, and a considerable proportion of them died.

TABLE continued.

Age.	Disease.	Blood.	Serum.	Bile.
28	intestines; inflammation of peritoneum and of small intestines; gall bladder distended with dark ropy bile	—	—	10491
25	A notorious drunkard; died of dysentery and abscesses of liver; large intestines inflamed, thickened and ulcerated; five small abscesses in the liver, containing a puriform fluid; the gall bladder distended with viscid bile, not distinctly morbid; the liver weighed 4lb. 13oz., and was softer than natural	—	—	10113
30	Dysentery, after ague; for some time they alternated; blood not buffed; serum of colour of Madeira wine; it was collected in two portions; first portion	10590	10330	—
	second portion	—	10316	—
28	Fatal dysentery; colon and rectum thickened and ulcerated; ileum inflamed; bile of natural appearance	—	—	10165
27	Fatal dysentery; cœcum ulcerated; colon ulcerated and gangrenous; ileum slightly inflamed; in right lobe of liver two abscesses, one holding about 8oz. of matter, the other about 2oz.; dark brownish bile in gall bladder destitute of mucus	—	—	10430
30	Dysentery, after intermittent fever; blood of natural appearance	—	10215	—
27	Fatal dysentery, complicated with abscess and gangrene of liver, not suspected during life; large intestines thickened and ulcerated; about three inches of lower portion of ileum severely ulcerated; a large cavity in the liver, its walls gangrenous, full of a foetid bloody fluid; gall-bladder distended with brown bile	—	—	10238
27	Fatal dysentery; liver of natural appearance; gall-bladder distended with thin, limpid, green bile	—	—	10107
23	Gun-shot wound in lumbar region; V. S. on 3d day; blood received in two portions; first, slightly buffed	—	10279	—
	second, free from buff	—	10278	—
	Gun-shot wound of lung. V. S. 4 hours after; blood of natural appearance	—	10295	—
	After about 10 hours V. S. repeated, when labouring under much dyspnœa, with pyrexia; blood not buffed	—	10275	—

TABLE continued.

Age.	Disease.	Blood.	Serum.	Bile.
20	Malay:—Gun-shot wound of lung. V. S. on 3d day; blood not buffed	—	10325	—
30	Singalese:—Gun-shot wound of neck, implicating trachea and œsophagus. V. S. on 3d day; blood not buffed; serum of the colour of porter	—	10303	—
22	Singalese:—Gun-shot wound of back. V. S. 6 hours after; blood of natural appearance	10508	—	—
30	Fatal gun-shot wound of pelvis and spine; the hepatic duct, in this instance exhibited an unusual peculiarity; just within the substance of the liver it was distended into a cyst, into which several branches opened, and from which a duct passed to the gall-bladder; it contained a good deal of thick, viscid, greenish mucus: there was healthy bile in the gall-bladder	—	—	10165
20	Negro:—Contusion of side from a fall; blood not buffed; serum reddish	—	10281	—
20	Singalese:—Contusion from a fall; blood not buffed	1058	10316	—
28	Fatal tetanus, after gun-shot wound fracturing both bones of fore-arm; tetanus began on 2d day; amputation was performed on the 3d, without benefit; death on the 5th; blood coagulated, not buffed; bile dark brown	—	—	1043

The observations contained in this second table were all made in the Military Hospital in Kandy, during the rebellion in 1818-19, when our troops were most actively employed in the field, were exposed to excessive fatigue and severe privations, often in most unhealthy parts of the country, and suffered dreadfully from disease, especially from those scourges of camps, remittent fever and dysentery. Some idea of the severity of the diseases may be formed from the fact, that the mortality

from disease alone, whilst the rebellion lasted, was about 25 per cent. of the strength. Besides the specific gravity of the blood or serum, in many instances I ascertained that of the bile; which I have thought it right to give, although not strictly belonging to the present subject.

All the cases referred to, with the exception of the Singalese, who were camp followers employed as coolies or porters, were men belonging to our regiments, serving in Ceylon, the Negroes and Malays to the 1st Ceylon regiment, the others to the 19th, 73d, and 83d foot. The Negroes were Africans, of Mozambique; the Malays were natives of Java, or the adjoining islands; the men belonging to the other regiments were either English, Scotch, or Irish.

As regards the general scope of the results, some plausible inferences perhaps might be drawn from them, in relation not only to the effects of disease on the circulating fluids, but also on the influence of climate, variety of race, &c. But I think it best to abstain from the attempt, considering the basis of facts as too slender for a foundation for theoretical views. As facts, however, bearing on an important part of pathology, few and imperfect as they are, I have thought it right to bring them forward, even after the lapse of twenty years, with the hope that they may not be altogether useless in themselves, and that they may lead to further inquiry.

3.—*Table Shewing the specific gravity of blood and serum in instances of disease in Corfu, and also the proportion of fibrin.*

TABLE V.

No.	Age.	Disease.	Blood.	Serum.	Fibrin per 1000 parts.
1	20	Remittent fever: blood of natural appearance	1060	—	1·7
2	22	Remittent fever: blood slightly buffed	1052	—	5·25
3	25	Chronic visceral disease of many months' duration. V. S. repeatedly performed	1047	1030	5·
4	23	Slight erysipelatous inflammation of arm: blood slightly buffed, and the first portion more than the second	1054 1053	1025 —	3·76 3·4
5	26	Epileptic fit preceded by rigor. V. S. to 24 oz. First portion had a pretty strong buffy coat: the last in a less degree	1044 1042	— 1023	4·78 3·7
6	25	The following day V. S. repeated: 2 lbs. abstracted, not buffed: the last portion Slight pain of chest without pyrexia after intoxication. V. S. to 3lbs.: blood slightly sizy: no distinct difference between first and last portion	1032 1055 1054	1021 1029 1027	4·1 1·3 1·3
7	20	Obscure abdominal disease (tumor) after fever. V. S. to 24 oz.: first portion slightly buffy: second portion in a less degree	1054 1052	1027 —	1·7 1·6
8	20	Sudden pain in region of liver with pyrexia when convalescent from fever. V. S. to 8 ounces, syncope.—Blood buffed in Sp. Grav. bottle, not in bleeding-cup	1038	—	3·4
9	25	Acute dysentery. V. S. to 2 lbs.: first portion not buffed, last portion, which flowed most freely, much buffed	1058 1057	— 1024	3·6 4·0
10	30	Acute Dysentery. V. S. to 26 oz. blood not buffed in the bleeding-cup, but distinctly so in Sp. Gr. bottle	1056	—	4·0

TABLE continued.

No.	Age.	Disease.	Blood.	Serum.	Fibrin per 1000 parts.
11	38	Intermittent fever, hot stage, with distressing retching and pain at scrobiculus cordis. V. S. to 1 lb.: blood slightly buffed in bleeding-cup, and distinctly in Sp. Grav. bottle: an appearance of air in the blood: it occurred in that which flowed last: a few small bubbles appeared to be disengaged, as it flowed into the cup, which was then close to the vein	1052	1025	3.9
12	26	Pain of side: little or no pyrexia. V. S. to 1 lb.: blood slightly buffed	1051	—	4.1
13	49	Rheumatic pains of three weeks duration: blood slightly buffed	1047	—	3.6
14	25	Dislocation of hip joint, from jumping out of window when drunk. V. S. to 4 lbs.: blood not buffed: serum subjected to distillation, yielded no perceptible traces of alcohol	1054		—
15	40	Catarrhal symptoms, with stupor after excessive drinking for five days.* V. S. to 2 oz.: blood slightly buffed: urine collected during the night, was of Sp. Grav. 1007: it was subjected to distillation: no traces of spirit could be detected in what passed over: it was of the specific gravity of distilled water.			
16	35	In perfect health, (for comparison)	1052	—	4.5

The observations in this table were chiefly made in the Military Hospital, in Corfu, and principally

* According to the man's statement, he drank about two bottles of spirits daily, besides beer, and porter. This case occurred in England, in an old soldier just after landing from India.

in the autumn. With one or two exceptions, the patients from whom the blood was taken, were soldiers of our regiments serving in the Ionian Islands. As regards salubrity, these islands may be considered as intermediate in degree between England and Ceylon; the average mortality in them amongst the troops being about 2·3 per cent. of the strength. Both the fevers and dysentery are less severe and fatal, than the same diseases within the tropics, and especially the latter.

The experiments were principally instituted with a view to endeavour to determine two doubtful points:—1st, whether there is any constant relation, between the specific gravity of the blood and its capability of yielding a buffy coat; and, 2dly, whether the proportion of fibrin is augmented in blood possessing this quality.

Relative to the first point, by one inquirer, it has been maintained that “blood which gives the fibrinous coat in a great degree, has a lower specific gravity than healthy blood;”^{*}—at the same time remarking, “that moderately sizy blood drawn from a person before he becomes weakened, has not commonly a very low specific gravity.”

The results of the trials contained not only in this table, but also in the two preceding tables, are not in accordance with the first conclusion. The only fair inference, it appears to me, that can be

^{*} Sir Charles Scudamore, *Essay on the Blood*, p. 35.

drawn from them, is, that there is no necessary, that is to say, no constant connexion between the specific gravity of the blood and the presence or absence of the buffy coat. Confining the attention to the last Table, it appears that in five instances in which the buffy coat was slight, the specific gravities were 1047, 1051, 1055, and 1054; in other five instances, in which the buffy coat was moderately thick, the specific gravities were 1044, 1038, 1052, and 1056; in one instance in which it was thick, the specific gravity was 1057; and in one, in which it was absent, (the blood of a person in perfect health) it was 1052.

It may be laid down, I believe, as a general rule, that the blood of persons labouring under acute diseases, differs in specific gravity very little from healthy blood; and is of comparatively high specific gravity, whether buffed or not,—or at least not below the mean: whilst on the contrary, the blood of persons labouring under chronic diseases attended with debility and wasting, is comparatively dilute, and of low specific gravity; and, this, though most frequently associated with the property of having a buffy coat, is not necessarily so.

Moreover, in connexion with the present subject, I would further remark, that I do not believe, that the time required for coagulation is in any wise constantly connected with specific gravity, as the same author asserts; his words are, “blood possessing the highest specific gravity coagulates the most

quickly," and "that which gives the fibrinous coat in a great degree is slowest in coagulating."* The last portion of blood which flows from the vessels of a slaughtered animal, is of less specific gravity than the first; yet it coagulates more rapidly. The blood of the patient No. 8, in the last Table, which was of the low specific gravity 1038, coagulated rapidly: on the contrary, I have known blood which differed very little from healthy blood, and which did not exhibit a buffy coat, remain liquid eight minutes after having been drawn, and not till the tenth minute was its incipient coagulation distinct.

When the serum and coagulable lymph are not viscid, and in consequence not capable of supporting the red particles, these particles fall down rapidly; if the fluidity of the blood is great, two minutes are sufficient for their subsidence an eighth or even a quarter of an inch, as I have many times witnessed, and instances of which I shall hereafter bring forward. No doubt, very buffy blood is very slow in coagulating, for the red particles cannot sink far in a very short time; and it is well known that healthy blood, which is of comparatively high specific gravity, coagulates rapidly. It is my wish to be understood to maintain merely, that the qualities in question,—high specific gravity and rapidity of coagulation; low specific gravity and slowness of coagulation,—are not necessarily connected, but rather accidentally, and consequently, that neither can be considered as

* *Op. cit.* p. 35.

a general fact, and be announced as a rule or principle.

Relative to the second point,—whether the proportion of fibrin is augmented in sily blood, as is the opinion of the same author,* the results of the trials, detailed in the last table, favour rather the conclusion, that there is no constant relation between the appearance of the buffy coat of the blood and the proportion of fibrin which the blood contains.

Sir Charles Scudamore, in the experiments from which he draws the conclusion, that sily blood contains a much larger proportion of fibrin than healthy blood, estimated the proportion of fibrin not for the blood as a whole, but for the crassamentum. Now, as sily blood commonly affords a contracted crassamentum in which the fibrin is comparatively condensed, and in which the residual proportion of serum is comparatively small, — it follows that a definite quantity of sily crassamentum *must* contain more fibrin, than the same quantity of healthy crassamentum, and yet the healthy blood *may* contain more fibrin than the sily blood.

In the fifth example in the last Table, the serum, was decanted from the crassamentum in the two successive instances of V. S., and the red particles were expressed from it: the following were the results,—estimated per cent.†

* Op. cit. p. 119, et passim.

† The colouring matter mixed with the serum of the clot was obtained by breaking up the clot in a moist linen bag, and subject-

Decanted Serum.		Expressed colouring matter and Serum.	Dried Fibrin.
1st V. S.—1st Portion	39·65	53·68	·478
2d Portion	44·6	42·8	·37
2d V. S.—2d Portion	30·3	30·8	·41

Now, doubtless, although the last portion of blood did not contain more fibrin than the first, yet, had the estimate been made not for the entire blood, but for the crassamentum, the proportion of fibrin in this last would have appeared to have been very much greater than in the first, in accordance with the preceding remark.

Regarding the results generally, the same remarks are applicable to them, as to those contained in the 2d Table ; and, independent of the particular application, they are brought forward with the same view. The only ones which appear to me to require any special notice are those connected with the Patients,

ing it to moderate pressure by means of the fingers, till little but fibrin remained. The fibrin was then well washed in the same bag until all the colouring matter was removed, when it was collected, and, previous to being weighed, dried on a platina capsule thoroughly at a temperature of 212°. And the same method in relation to the fibrin was used in every instance in which the proportion of it obtained is given in the Table.

Nos. 14 and 15, from whom blood was taken after excessive drinking of spirits.

In these two instances I have mentioned, I could not detect alcohol; in one case in the serum of the blood, and in the other in the urine. It is an interesting and important question, what becomes of the spirit taken into the stomach. Should other experiments confirm the above, the unavoidable inference seems to be, that it is decomposed.*

8.—*Observations on the Formation of the Buffy Coat of the Blood.*

In the preceding pages, I have endeavoured to shew, first, that there is no necessary connexion between the buff on the blood, and the specific gravity of that fluid; and, secondly, that there is no necessary connexion between the quantity of fibrin in the blood, and its tendency to exhibit the buffy coat.

The circumstances, or qualities, on which the formation of the buffy coat depends are, I believe, chiefly those so sagaciously and ingeniously pointed out by Hewson, and principally confined to the coagulable lymph, little dependent on the serum and red

* Recently, I am informed, in instances of death from alcohol, either taken by men in excessive quantity, or introduced into the stomach of animals, it has been detected in the blood, but in a larger proportion in the brain. The author of these interesting results is Dr. Percy, who, greatly to his credit, obtained them whilst a student at Edinburgh, during (if I have been rightly informed) the past year, 1838.

particles which may be considered as passive, in a manner, rather than active.* The circumstances or qualities alluded to, belonging to the lymph, are its coagulating more slowly than in healthy blood, and its possessing greater tenuity or liquidity, not only than lymph in its healthy state, but even than serum; in consequence of which the red particles more readily subside, and the lymph coagulating, free from the presence of colouring matter, appears colourless, constituting the buffy coat.

Of late years, this view of Hewson's relative to the formation of the buffy coat, has commonly been only partially adopted, and the circumstance, viz. the slowness of coagulation, which he held to be least important, has been most insisted on, and has been considered most essential. Even Professor Müller is not an exception in this respect; in his *Elements of Physiology*,† he enumerates as the principal causes

* As the serum of the blood and its red particles appear to be subject to some variations, they may occasionally in a minor degree influence the production or prevention of a buffy coat. The serum is naturally in a certain degree viscid; an increase of this quality, *cæteris paribus*, would retard or prevent the appearance of the buffy coat, and *vice versâ*. The red particles are naturally of comparatively high specific gravity, owing to which they rapidly subside, in a fluid of no or of little viscosity; any diminution of this quality, whether from expansion of the corpuscles, as from a kind of dropsical state, or from greater buoyancy from adhering particles of air, also, *cæteris paribus*, would have a similar retarding or preventing effect on the formation of a buffy coat; but, let me remark, these are hypothetical considerations.

† *Elements of Physiology*, part i. p. 117, translated by Dr. Baly.

of the subsidence of the red particles and the formation of the buffy coat in inflammatory blood, not an increased tenuity of the coagulable lymph, but the slow coagulation of the blood or lymph, and an increased quantity of fibrin.

In a paper on this subject which was published in the Philosophical Transactions for 1822, I endeavoured to call attention to Hewson's original views, and to show, that in the production of the phenomenon in question, diminished viscosity is more concerned than diminished coagulability, using the expression in relation to time. And in support of this theory, I adduced some instances, in which the buffy coat appeared on blood which was not marked for slowness of coagulation. I stated, that in certain cases "in which the inflammatory diathesis is best marked, the separation of the red particles from the blood drawn is most rapid, often occurring in one or two minutes ; and, that in some diseases, particularly erysipelas, the blood taken from a vein coagulates as rapidly as healthy blood, and yet exhibits the buffy coat. In instances of this kind (it was remarked) when I have watched the coagulation of the blood, the red particles have subsided in the short space of two minutes, leaving a supernatant stratum of coagulable lymph, transparent and liquid. The buffy coat, in these instances, did not appear on the blood collected in the common bleeding cups—only when small vessels, as wine-glasses or small gallipots, were used, and quickly filled and set aside to rest."

And, I added, as a general inference from the observations also in conformity with Mr. Hewson's views, "May it not be inferred generally, that the buffy coat is principally owing, not to the slow coagulation of the blood on which it appears, but to its increased tenuity, or, in other words, to the diminished viscosity of coagulable lymph, the effect of morbid vascular action, connected with the inflammatory diathesis?"

Mr. Hay, in his "Observations on the Blood," had objected to Mr. Hewson, that the coagulable lymph is not itself attenuated in inflammatory diseases, and that when it appears to be so, it is from dilution with serum. Were this opinion correct, I remarked in the same paper, such blood should be of a specific gravity unusually low, which it is not, as I had satisfied myself by numerous experiments, made both at home and in Ceylon, and of which experimental proof has already been afforded.

9.—*Historical Notices respecting the Blood, particularly its Coagulation.*

A carefully drawn-up history of the speculations which have been formed, and of the observations which have been made by ingenious men, relative to the coagulation of the blood, and the formation of the buffy coat, would be curious and interesting in many respects; and, it may be noticed as a desideratum in medical literature.

Aristotle appears to have attributed the effect of coagulation to the presence of a fibrinous matter in the blood, either in the state of minute solid fibres, or liquid with the power of forming fibres. From an incidental expression,* it is not improbable that the latter was his opinion; and that he considered the matter liquid whilst the blood was in motion in the living animal, like the matter of curd in uncoagulated milk; and, solid only and fibrous, after extraction, the coagulation being the effect of the change.†

* Meteorol. lib. iv. c. 7. D.

† Harvey thus gives the opinions entertained by Aristotle respecting the blood: "Aristoteles (*a*) quoque ut sanguinem alendi gratia institutum putavit, ita eundem etiam veluti è partibus compositum censuit. Nempe ex crassiore et atra, quæ in fundum pelvis inter concrendum subsidit, eaque pars illi deterior habetur (*b*): sanguis enim, inquit, si integer est, rubet et dulcis saporis est; sed, si vel natura vel morbo sit vitiat, atrior cernitur. Ex parte etiam fibrosa sive fibris constare voluit: iisque demptis, ait (*c*), sanguis neque concrevit neque spissatur. In sanguine præterea saniem agnovit: Sanies, inquit, sanguis incoctus est; aut, quia nondum percoctus, aut quod in seri modum dilutus fuerit. Atque hunc frigidior esse ait: fibras autem partem sanguinis terrenam esse statuit." (*d*)

There are other particulars mentioned by Aristotle respecting the blood, not uninteresting in the history of this fluid, and clearly shewing what great attention he had paid to it:—the following are some of the more remarkable.

"Sanguis in primis necessarius communisque sanguineis omnibus animalibus est, nec adventitius suppetit, sed vernaculus, atque intimus in omnibus integris, atque imputidus habetur. Venas (he included the aorta and its branches) hic vasa sibi, et conceptacula

(*a*) De Part. Anim. lib. ii. c. 3.

(*b*) De Hist. Animal, lib. iii. c. 19.

(*c*) De Hist. Animal, lib. iii. c. 19.

(*d*) De General. Animal. 402. (Op. Om. a Col. Med. Lond. Edit.)

Harvey appears to have adopted an opinion on the subject very similar to that of Aristotle;* as did

habet; nec in ullo alio nisi in corde, præter venas continetur. Tactum nullo in genere sentit; sicut excrementa quoque in alvo contenta, sensu carere certum est. Quinetiam cerebrum et medulla tactum non sentiunt. Copia sanguinis in primis animalibus, iis, quæ animal et intra se formant, et in lucem edunt: mox iis sanguineis, quæ pariunt ova. Palpitat intra venas sanguis omnium animalium pulsuque simul undique movetur, solusque omnium humorum sparsus per totum corpus animalium est. Et semper quandiu vita servatur, sanguis unus animatur et fervet. Oritur primum in corde, antè, quàm totus corpus formatur. Si multum sanguinis effluat, anima deficit: si nimium, vita interit. Si sanguis immodicè humescit, morbus infestat: sic enim in speciem saniei diluitur, et adeo serescit, ut jam nonnulli sudore cruento exundarent. Idem etiam interdum causæ est, ut qui effluxerit, aut omnino concreescere nequeat, aut incomptè, particulatimque spissatur. Dormientibus porro sanguinis copia partes exteriores deserit, subitque interiores, ita ut adacto cultello non æque effluere possit. Minus sanguinis per summa corporis fæminarum est: plus enim parte interiore continetur, et quæ menstrua appellamus, plurima fieri in mulieribus solent: quod genus sanguinis vitiatum, ægrotansque, fluit immodicè, atque profluvium ob eam rem dictum est. Cæteris morbis sanguinis minus mulieres infestantur, quam viri. Paucis item mulieribus fiunt mariscæ, aut varices, aut è naribus profusio sanguinis. Et si quid eorum acciderit, detrimentum in menstrua decumbit. Ætatum etiam ratione sanguis discrimen, tum in modo, tum in genere recipit. Etenim in ætate admodum juvenili saniem trahit, et largior est: in senectute autem crassus, niger et paucus. Medium tenet in media, firmâque ætate. Concrescit etiam facilè senum vel in corpore sanguis, qui per summa est: quod idem nunquam juvenibus evenit. De Hist. Animal, lib. iii. c. xix.

* In the following passage, Harvey appears to give his opinion on the nature of the blood, founded on that of Aristotle; but modified, and approaching a little nearer to the present state of our knowledge on the subject. "Hic enim (sanguis) ut est naturale quid, heterogeneum sive dissimilare existens, in succis aut partibus illis componitur. Prout autem vivit, parsque animalis præcipua est, ex

Sydenham also some years later ; but with this difference and refinement,—an anticipation of a recent doctrine already abandoned, that the buffy coat is formed of the substance of the colouring matter, deprived of its pellicle.*

Amongst the distinguished physiologists, the immediate successors of Harvey, Lower holds a high place ; and he appears to have been one of the earliest, who had a more precise and a more accurate knowledge of the blood, than was then current, or had descended from antiquity. He seems to have considered the serum of the blood in the light

succis iis simul mistis constans ; est pars similis animata ex anima et corpore composita. Evanescens autem, ob extinctum calorem nativum, illa sanguinis anima, substantia hujus nativa illico corrumpitur et dissolvitur in ea, ex quibus olim constituebatur : primo scilicet in cruorem, postea in partes rubras et albas ; partesque rubræ superiores sunt floridiores, inferius autem sitæ nigricant. Partes porro aliæ fibrosæ sunt et densiores (reliquarum vesiculum) : aliæ ichorosæ et serosæ, quibus coagulatus thrombus innatare solet. Atque in hoc serum sanguis tandem fere totus degenerat. Partes autem istæ non insunt vivo sanguine, sed a morte solum corrupto et jam dissoluto.”—Op. Cit. p. 404.

* Sydenham thus expresses himself on the subject of the buffy coat : “Sanguis in pleuritide saltum ille, qui post primam vicem extrahitur ubi refrixerit, sevi liquati præ se fert speciem ad crassitiam satis conspicuam, ac superficiem habet veri *puri* æmulam, et tamen ab eo longe diversam utpote quæ fibris, instar reliqui sanguinis, arcte contextitur, nec non ad modum puris diffluit, quin à reliquo divulsa discolor illa pars, formam cuticulæ tenacis et fibris refertæ exhibit ; et, fortasse nihil aliud est, quam fibræ sanguineæ, quæ rubicundo ac naturali suo integumento per præcipitationem exutæ, ambientis aëris frigore in membranam hujusmodi subalbidam concrevere.”—Op. Univers. Lug. Bat. 1741. p. 264.

of a vehicle: “*propter seri defectum, sanguis, vehiculo suo spoliatus, circulatione ineptus redditur.*”* He was not only acquainted with the coagulability by heat of serum; but also of the liquor pericardii, and of lymph,—“*lymphæ glandulis secreta.*”† He appears to have been well acquainted with the existence, in the blood, of a glutinous fluid, its nutritive and its coagulable part,—to which the coagulation of the blood is owing, and many morbid phenomena are referrible; thus, at page 130 of his work on the heart, he speaks of it, as “*succus nutritius in sanguine admodum glutinosus;*” and which “*sensim concrescit, et pro cordis et vasorum continentium figurâ, variè configuratur, unde pro cordis polypo, verme et serpente interdum habitus est.*”

Borelli about the same time, or shortly after, entertained the same view of the blood, and announced it with even more precision, and illustrated it by experiment.‡ He had come to that conclusion also, which I have inferred Aristotle had formed,—that the glutinous part of the blood, in the living animal, is liquid; he was led to it by considerations connected

* *Tractatus de Corde*, p. 131.—London, 1669.

† *Op. cit.* p. 6.

‡ “*Primaria pars rubicunda constat ex glutinossissima quadam substantia lubrica, et ex succo quadam purpureo, qui simul implicati spontè concrescunt in grumos; et post concretionem crebris aquæ ablutionibus apparet pars glutinosa, constipata in fibras candidas, vel in membranas reticulares similes tendinosis, ut in sanguine columbino patet; pars verò rubra grumefacta, et abstersa ab aqua, abit in pulverem rubicundissimam, in fundo aquæ subsistentem.*”—Borelli *de Motu Animalium*, p. 192, (1685).

with microscopical observations: “ideo fatendum est, gluten album sanguineum lubricam et fluidam consistentiam retinere dum in animale vivente movetur.”

These just views may have been adopted by a small number of individuals, but they seem to have made no general impression at the time, or to have been objected to, or rejected, as if erroneous.* For a considerable period after Harvey’s great discovery,—for nearly a century, whilst many branches of physical science were most successfully cultivated, comparatively little advance was made in the knowledge of the true nature of the blood. The attention of the philosophical chemists was then chiefly directed to the effects of heat on it, and to the products obtainable by distillation;† whilst that of the natural philosopher and physician was directed, prin-

* Haller, in his *Comment. on Boerhaave*, vol. ii. p. 310, published in 1742, referring to the opinion that the clot contains fibrous matter,—the opinion he says, of Malpighi, Michelotti, Guglielmini, Hacquet, and of Adams; observes, “Verum, ne errori nascatur locus, fibrosa ea substantia non alia est ab ipsis globulis dictis (*i. e.* of Leeuwenhoeck) et evanescit solo calore.”

† Macquer, one of most philosophical chemists of his time, in his *Elements of Chemistry*, published towards the middle of the last century, speaks of the analysis of the blood, as “a sort of general, though imperfect, analysis of the animal.” Speaking of the blood out of the body, he notices only “its yellowish serum or lymph,” and the red curdled part which swims in the midst thereof. He adds, “these two substances, when analysed, yield nearly the same principles; and, in that respect, seem to differ but little from each other.”—*Elements of the Theory and Practice of Chemistry*, Eng. Trans., 1764.

cipally, almost exclusively, to its red particles, to which they attached vast importance, making their microscopical observations the foundation of various speculations and wide-reaching hypotheses.*

At what time exactly the more precise ideas on the subject began to prevail,—those which are inculcated in the writings of Hewson and the Hunters, and some of their able contemporaries, it is not easy to determine. The term “coagulable lymph” was then in common use, according to Hewson’s definition of it; thus, Sir John Pringle, in the description of a fatal case, in which the heart was found to adhere to the pericardium, says, it was “in such a manner as parts adhere from recent inflammation, that is, by an exudation of coagulable lymph.”† The probability,

* Boerhaave, opposing the hypothesis of Galen, that the blood is composed of four peculiar humours, remarks, “it appears clearly from the experiments of Leeuwenhoeck, that the red clot (the first of Galen’s Elements) is composed merely of red particles united so as to form one mass; that the yellow bile (his second Element) is merely serum, and that its more dilute and pellucid parts (his third Element, in which the yellow bile was supposed to be dissolved) is to be considered as phlegm; and that the black parts of the crassamentum, its inferior portion (his black bile, and fourth Element) does not differ from the red, excepting in not being exposed to the air.”—*Prælect. Acad.* I., 328. The elaborate medical doctrines which this distinguished man built up, on the incorrect microscopical observations of Leeuwenhoeck, afford the most remarkable instance of what has been stated in the text; and should ever be a warning in refined and obscure inquiries, in which the microscope is brought into use.

† Hewson, in his *Treatise on the Lymphatic system*, published in 1764, gives the case at page 114, transcribed from Sir John Pringle’s notes.

however, it appears to me, is, judging from the writings of Jurin,* Haller,† and Marherr,‡ that the views on the subject of the blood entertained by our great English physiologists last mentioned, (passing by the earlier and similar opinions, which never took root, and seem to have been lost), belonged to their time, and chiefly to our country; and that to them,

* Dr. Jurin, in his able paper, in the Philosophical Transactions for 1719, on the specific gravity of the blood and its different parts, distinguishes only the red particles and the serum. He was of opinion that the crassamentum is composed “of the globular particles united together by their attractive power.”

† Haller, in the second volume of his *Elementa Physiologiæ*, published in 1760, at page 42, remarks,—“Solent Medici Galli superiores (Petit Mem. de l’Acad. des Scien. 1732, p. 393; Senac, t. ii. p. 92,) monere, duriora coagula esse, quæ ex sero nascuntur, et serum vi densabili cruorem superare, cum in frigida coeat, dum sanguinis pars rubra conservat.” “Verum certum est,” he continues, “etiam ex rubro cruore, dura nasci coagula, et quidni nascantur, cum in ea sanguinis parte, et terræ et ferri, plurima sit portio.” So vague is this great physiologist on the subject.

‡ Marherr, in his admirable “*Prælectiones Medicæ*,” published in 1772, probably expressed the doctrines then current, and considered most orthodox, in the continental schools, at least in the North of Europe; he attributes the buffy coat of the blood to a change in the serum. “Augetur vero visciditas illa seri sæpe tantum ut etiam more cruoris protinus coagulatur et ipsum dum sanguis de vena missus coit. Atque tum cruor concretus pellicula alba densa, tenace tegitur, quæ ex sero viscidior nascitur, et crustæ pleuriticae nomine insignitur, quoniam,” &c., tom. ii. p. 254. The crassamentum, he considered, composed of red particles adhering together; “thus,” he says, “coagulum hoc rubrum minui sensim mole et mora temporis in vase serosum liquidum augeri, quod antea in interstitiis coaguli interclusum, per moram et quietem magis sese attrahentibus moleculis rubris, ex hisce interstitiis exprimitur, dum illæ ad se proprius accedunt.”

and perhaps to one or two French inquirers, belong the merit of reviving, or rather of originating, them *de novo*, and of proving them true.

One or two remarks of Hewson's seem to point to this, and in a manner claim the honour of discovery; thus, in a foot-note, at page 51, of his "Experimental Enquiry into the Properties of the Blood," after having detailed some of the most remarkable of his results, he observes, "that most of the facts which occur in the preceding pages have been mentioned in my Anatomical Lectures, ever since the year, 1767; and some of them were mentioned publicly even before that time." And he adds, "This I thought it necessary to observe, because many of them have since appeared in other publications." Thus, in another place, also in a foot-note, after describing the crassamentum, which, he says, "it is well known consists of two parts, of which one gives it solidity, and is by some called the fibrous part of the blood, or the *gluten*, but by others with more propriety termed the *coagulable lymph*," remarks, "It may be proper to mention here, that till of late, the coagulable lymph has been confounded with the serum of the blood, which contains a substance that is likewise coagulable. But in these sheets, by the *lymph* is always meant that part of the blood which jellies, or becomes solid spontaneously, when blood is received into a basin, which the coagulable matter that is dissolved in the serum does not, but agrees more with the white

of an egg, in remaining fluid, when exposed to the air, and coagulating when exposed to heat, or when mixed with ardent spirits, or some other chemical substances.”*

The views of Hewson, on the subject of coagulable lymph, on the coagulation of the blood, the formation of the buffy coat, the contents of the lymphatics, on the analogy between the coagulable lymph which they contain and the coagulable lymph of the blood, are so clear and precise, and deduced from such admirable experiments, that it appears not a little difficult to comprehend, how they could be either over-looked or not perfectly understood or appreciated. And yet, that they have been, is the unavoidable impression received in reading some of the works of the able physiologists of the present day. Justice has not yet been done to the high merits of Hewson, either as a most acute and accurate observer, or as a very original inquirer. Dr. Baly, in his translation of Professor Müller's work on Physiology, in a note to that part of the text, in which it is said, that Berzelius suggested the idea that the clot is composed of fibrin enclosing the red particles, supports the just claims of his countrymen, and in part those of Hewson, but not, I think, to the full extent of his merits. Hewson's ideas on the nature of coagulable lymph, founded on the results of his experiments and observations, appear to me as clear as possible—even as clear as those of Professor Müller himself, and that

* Op. cit. p. 6.

he would have made the same ingenious experiment in confirmation of them (I allude to the separation of the fibrin of the blood of the frog from its red particles, by filtration in its liquid state), had he been aware, that these particles, on account of their large size, could be detained on a filter.

As Dr. William Hunter was senior to Hewson, and as the latter was associated with him, it may be a question, whether the merit of the discovery of coagulable lymph, is not due to him.

In his controversy with the late Dr. Monro, published in 1777, under the title of "Medical Commentaries," he gives a quotation from his Lectures, delivered in 1759-60, from which it appears, that then he had a clear apprehension of the part performed in the blood by coagulable lymph. I shall transcribe the passage, as it is on many accounts interesting. Accounting for the red blood not transuding through the vessels, he refers it principally to the glutinous quality of the blood, when "equally mixed up with its coagulating part." "That part," he adds, "coagulates as certainly as the blood stagnates, even in living bodies; and when the universal stagnation happens in death, this part of the blood collects itself into irregular *polypi* and coagulations all over the body, and the rest of the blood is no longer the thick viscid fluid it was before, but rather a bloody *serum* that will ooze through all the vessels and membranes."

Now as Hewson, at this time was only about 20

years of age (according to Mrs. Hewson, he was born in Nov. 1739, and was in partnership with Dr. Hunter in 1768*), Dr. Hunter's knowledge of the nature of coagulable lymph could hardly have been derived from Hewson, and it must be considered either as the result of his own observations, or of observations common to him and his contemporaries, and not claimed by any one in particular, which, on the whole, perhaps, is most probable. Still, however, to Hewson I believe belongs the merit of elaborating the doctrine, and of making precise what was before vague.

In mentioning the revival of correct ideas on the subject of the coagulation of the blood, I have alluded to one or two French inquirers, who perhaps may be considered entitled to share in the merit I have attributed to our English physiologists, if not to have anterior and superior claims, particularly those very able and distinguished men, Petit and Senac. They certainly preceded Hewson, in adopting Borelli's view, or the analogous view alluded to, of the nature of coagulation of the blood, as solely depending on a white coagulable part, which they called lymph; but I am not aware that either of them brought forward in support of it any facts, or experimental results more decisive than those of the Italian physiologist; nor do their statements appear to have had any influence on the doctrines of the day, judging from the opinion of Haller, already

* Biograp. Med. 1. 428.

alluded to. Below, I shall give some extracts from their writings, expressing their ideas on the subject.*

* Petit, in his second "*Mémoire sur la manière d'arrester les Hemorragies*," published in *Mem. de l'Acad. des Sciences* for 1732, makes the following remarks:—

"Attendu que la lymphe est plus légère, la moitié superieure de ce caillot est blanche, et l'inférieure est d'un rouge-brun."

"Si l'on examine le bassin dans lequel on vient de saigner du pied, on trouvera toutes les parties du sang noyées dans l'eau chaude, et si l'on veut voir à l'instant quelle est la partie du sang susceptible de coagulation, on n'a qu'à jeter un pot d'eau froide dans le bassin, et sur le champ, on verra la partie blanche se séparer de la partie rouge, et s'élever sur la surface de l'eau où elle forme des caillots très-durs, pendant que la partie rouge demure universellement et exactement mêlée avec l'eau, et sans former aucuns caillots.

De ces expériences connues de tout le monde, on peut conclure que la partie blanche est non-seulement plus disposée à la coagulation que la partie rouge, mais qu'elle est la seule qui se coagule, et que la partie rouge ne feroit point partie du caillot, sans la partie blanche qui la retient. Les différents degrés de consistance qu'on trouve dans les caillots en sont une seconde preuve. En effet, le caillot de toute la masse exactement mêlée a quelque consistance, mais lorsque la partie rouge et la partie blanche se sont coagulées, pour ainsi dire, séparément; le caillot blanc est très dur, parce qu'il ne contient point de partie globuleuse, et le rouge est d'autant plus mol qu'il contient peu de lymphe; du manière que quand la partie globuleuse et la sérosité restent fluides, et que la lymphe se coagule, le caillot est encore plus dur et plus blanc: ainsi les différents degrés de blancheur et de solidité des caillots dépendent du plus ou du moins de parties globuleuses que la lymphe retient, en se coagulant."—p. 393.

Senac, in the first edition of his *Treatise on the Heart*, published in 1749 (vol. ii. p. 660), speaks of lymph, incidentally, in connexion with the colouring particles, when refuting some errors of Leeuwenhoeck relative to the latter. He mentions it as "*cette matière blanche, qui se coagule d'elle-même et qui forme la cœne du sang des pleurétiques*." He continues, "*J'ai d'abord voulu l'examiner quand la sang s'en sépare et se refroidit; mais on ne peut pas la saisir exactement ou elle est encore fluide. J'ai donc pres cette matière figée et fort*

Should the historical desideratum to which I have alluded, in introducing these brief notices, be supplied, written with the scientific accuracy and philosophical spirit suitable to the subject, it will prove, I apprehend, no less curious than instructive: the

blanche; mais je n'y ai vu aucune trace des globules." He does not claim to himself the discovery of the coagulability of lymph; more than he does the globularity of pus, the globules of which, as seen under the microscope, he correctly describes as larger than the blood particles.

The next passage I shall transcribe is from the second edition, which appeared in 1783, after the death of the author; he thus opens the 13th chapter of the 5th book, on the causes which keep the blood liquid, "Cet assemblage de matières inégalement pesantes, n'est qu'un fluide artificiel, ou pour mieux dire ce n'est pas un corps fluide; il n'est qu'une matière fondue par diverses causes; sa liquidité ne subsiste qu'autant que ces causes agissent; abandonné à lui-même il se coagule dans ses propres vaisseaux; si on lie une carotide en deux endroits, dans un animal vivant, le sang se condense entre les deux ligatures; si les issues du cœur ne sont pas libres, il peut se former des concrétions dans les ventricules; elles ne sont pas rares dans l'aorte, lorsqu'elle se dilate.

Toutes les parties du sang ne forment pas par elles-mêmes le fonds de ces concrétions; c'est la lymphe seule qui se coagule, et qui, en se coagulant, est le lieu des autres matières qui y sont mêlées; elle lie, par exemple, avec elle les molécules rouges et leur donne de la consistance; si elles étoient seules elles ne s'attacheroient point les unes aux autres; dès qu'elles sont séparées du reste, elles flottent dans l'eau; ou si elles se rapprochent, leur cohésion est légère; elles retiennent même quelque portion de matière lymphatique qui les unit; aussi leurs concrétions sont elles tendres, et semblables à une gelée presque liquide."

Hewson's Experimental Inquiry into the Properties of the Blood, was published three years before the last edition of Senac's work, viz. in 1780, with the very characteristic motto, both as regards the inquiry itself and the author's own mind, "*Vere scire, est per causas scire,*" words of the author of the *Novum Organum*.

history of this our vital fluid, will be any thing but a triumph of human intellect; it will shew how difficult is the attainment of physiological knowledge, (considering merely the coagulation of the blood,—as it were, a single quality;) how insecure is the tenure of this knowledge; how unstable and fleeting is opinion; and how prone the philosopher has been to adopt wild and fantastic notions; and to fall into almost absurd errors, when deviating from the rigid inductive method.

II.

MISCELLANEOUS OBSERVATIONS ON THE BLOOD.

Most of the following observations were made between the years 1824 and 1833, in the Mediterranean, chiefly in Corfu and Malta; and were published at intervals in the Edinburgh Medical and Surgical Journal. They are now given in many instances more in detail, on the persuasion, that to be useful, detail is necessary, even at the risk of tediousness.

1.—*On the Effect of Violent Agitation on the Blood.*

Dr. Bostock, in the first volume of his Elementary System of Physiology, remarks, “It is well known that if blood, as it is discharged from the vessels, be briskly stirred about for some time, the process of coagulation is entirely prevented from taking place, either in consequence of a more complete union of its parts with each other, which prevents their future separation,—or from the fibrin after it has been for some time discharged from the blood,

losing this peculiar property by which its particles are attracted together.”*

To ascertain, if a brisk motion really prevents the coagulation of the blood, as is asserted by Dr. Bostock, I made the following experiment, preferring it to the means mentioned by him, as we know that by moderately stirring the blood, its fibrin merely is separated, and not its property of coagulating destroyed. About two ounces of blood were received into a large vial and immediately shaken violently, and the agitation was continued without intermission for ten minutes, which was two minutes after the blood at rest coagulated. The result was, that the blood thus shaken, acquired a scarlet hue from being mixed with, and from the action of air, and *appeared* to be liquid. But this was merely in appearance; for when poured on a filter, it separated into two parts,—one, the serum, containing, suspended in it, the red particles, which passed through the filter:—the other, the fibrin in a finely divided state, coloured by adhering red particles which remained on the filter. This effect of agitation may be imitated by breaking up the crassa-

* Lower supposed that agitation had this effect on blood, and in his time, (now at least 170 years ago) the opinion appears to have been current: thus, when on the subject of transfusion, and considering the best state of blood for the purpose, he gives the preference to that flowing from a living animal, because, he remarks,—the blood which has been collected in a vessel, (*utcunque satis præcaveri posset, crebrâ agitatione, ne congeletur*) is necessarily in some respects changed.—De Corde, p. 173.

mentum of the blood with the fingers, when an apparent solid is rendered an apparent liquid. Nor is this at all surprising, considering the small quantity of solid matter contained in the crassamentum, to which it performs the part, as it were, of a cement, or of calcareous matter to sand, combined with which the sand constitutes a solid rock, and deprived of which it is a yielding quicksand, almost as fluid, as the red particles of the blood deprived of fibrin.

In confirmation of this explanation, I may add, that blood rendered apparently liquid as above described, when examined by the microscope, is found to contain, besides the blood-discs, which are little altered* floating in the serum,—masses of fibrin, of irregular form and structure, — (neither distinctly globular nor fibrous) and particles also of irregular form, very much smaller than the blood-discs. On rest, after a few hours, a separation takes place,—the red particles, in consequence of their greater specific gravity, subside towards the bottom, and the particles of fibrin, owing to entangled air, collect at the surface.

2.—*On the Effect of moderate Agitation on the Blood.*

Sir Charles Scudamore in his Essay on the Blood, endeavours to shew that moderate agitation of the

* The discs after agitation are not quite so regular in form as before.

blood favours its coagulation,* and, conceiving that his experiments prove the fact, he accounts for it on the idea, that agitation accelerates the escape of the carbonic acid gas, which, as he believes, the blood contains, and on the escape of which, he is of opinion the phenomena of coagulation depend.

It may appear a most easy matter to determine whether moderate agitation does, or does not accelerate the coagulation of the blood, the experiment is so simple, and so easily made; and, yet I must confess I have experienced considerable difficulty in arriving at a conclusion, from the contradictory nature, not of one or two, but of many results. The great difficulty in this inquiry, and in most inquiries relative to the blood, is, the impossibility of knowing that any two portions of the same fluid are exactly similar. My opinion is, that no two portions are, and even that no two drops of this fluid are exactly similarly constituted, or possessed of exactly the same properties; and that the portions of a continuous stream flowing from the arm, were it divided into a hundred, may all be different. The results of experiments first led me to adopt this notion, in which I was confirmed by further experiments, and by reflecting on the subject. Not to be tedious, I shall mention only one experiment. Six gallipots of the same size, and a platina crucible of nearly the same size, were filled in succession with venous blood as it flowed from the arm

* Op. cit. p. 40.

of a soldier labouring under a slight ailment, and on whom no well-marked effect connected with loss of blood was perceptible during the operation. They were placed close to each other, and carefully observed. The blood in each did not coagulate, as might have been expected, in the order in which it was received, but irregularly. Thus the first received was the last but one to coagulate, and that which was received in the crucible, which was the fourth in succession, coagulated first. When one reflects on the subject, the inference appears to me to be obviously what I have suggested above, viz. that no two drops of blood are precisely similar in their composition and properties. It is evident on reflection that the fluid of a continuous stream of blood must be derived from different parts of the body. In one portion there may be an unusual or less than usual quantity derived from the vessels of the muscles, or brain, or liver, or spleen, &c.; and we cannot but suppose, that the blood from the kidney, after urine has been secreted from it, is different from the blood from the liver or pancreas, after supplying bile and the pancreatic fluid. The preceding experiments and this reasoning are confirmed by another fact which I have observed; viz. that if the crassamentum, the instant it is formed, is removed, it will be succeeded by another coagulum; and if this is removed, by another,—and so on repeatedly till all the fibrin has coagulated,—clearly demonstrating, that in any given quantity of blood,

there are particles of fibrin of various degrees of rapidity of coagulation. And is it not also confirmed by microscopical observation? If we examine, with a powerful microscope, a minute portion even of a drop of blood, as indeed is necessary; is there not a variety observable in the particles, either as regards the outlines of the discs, their thickness, or their other dimensions? The resemblance of one to the other is general and considerable; but not without, as it has appeared to me, slight differences.

To return now to the effect of moderate agitation on the coagulation of the blood,—it may be easily understood how seemingly contradictory results may be obtained, if the effect is not very well marked. It certainly is not well marked. In some instances it has appeared rather to retard; in others neither to retard, nor accelerate, and in others, and these the majority, to accelerate coagulation.

As the majority of the experiments which I have made indicate this, I infer that agitation does promote the coagulation of the blood out of the body. But admitting it to be a fact, I cannot adopt Sir Charles Scudamore's explanation of it, as it does not appear to me proved that any carbonic acid gas is disengaged from blood in the act of coagulation; or if it were, that its removal would have the effect imagined. I believe it is owing to a very different cause. I find that when a small portion of crassamentum is put into blood, the

coagulation is distinctly accelerated, having, as it would appear, an effect similar to that of a crystal in a saturated saline solution ready to crystallize; and it has occurred to me that agitation may promote coagulation by producing a more intimate mixture of the particles, of those which are first coagulated, with those which are still liquid; I say "first coagulated," because it may be inferred from what has been already stated, that some particles of lymph are coagulated almost immediately after the blood is drawn, and these may act on the mass like a portion of crassamentum introduced into it.

3.—*On the Effect of Lightning on the Blood.*

It is commonly believed that the blood of those killed by lightning does not coagulate. The opinion has been adopted by Hunter,* but on what evidence is not stated. Now, as it appears, that the blood of animals killed by the discharge of an electrical battery is not deprived of its power of coagulating, and as electricity, applied as a chemical agent, seems rather to promote than retard the coagulation of the albuminous contents of the blood, *à priori*, it does not seem probable that the popular belief is correct. The question, however, cannot be determined excepting by an appeal to facts. The following case, with this view, may be deserving of being recorded;

* Treatise on the Blood, &c. p. 34.

I shall give it in detail, from my notes taken at the time, on account of the interesting nature of the subject.

During a violent thunder-storm at Malta on the 15th of November, 1832, a Maltese labourer, aged 65, previously in good health, was struck dead by lightning. The body was conveyed to the Civil Hospital, where I had an opportunity of examining it twenty-four hours after the event.

The hair on the left side of the chest was slightly singed. The cutaneous vessels generally were very much distended with blood; the face and neck were purplish red; the inferior surfaces purplish. The fingers were contracted and *rigid*. The abdomen was very much distended, tense and tympanitic. There was froth at the mouth, mixed apparently with some of the discharged contents of the stomach.

On inspecting the abdomen, the stomach and intestines were found enormously distended with air. There was much air in the cellular structure from putrefaction; the cellular structure even in which the gall bladder is embedded in the liver, was distended with air. There was a general staining of the different structures by the blood, and the blood-vessels, and the lining membrane of the heart and bronchia was dyed dark-red. There was frothy blood in the veins, and in the right cavities of the heart with a little soft coagulum. The lungs were very red and distended with air, and its vessels were strongly stained.

The atmospheric temperature during the time that elapsed between the event and the examination, and the temperature of the room where the body was deposited, varied between 55° and 60° . Such advanced putrefaction as was witnessed, is even uncommon in the time specified, in the hottest weather. At the moment,—reflecting on the degree of putrefaction, and the state of the blood, I had little doubt, that the latter was owing to the former, however that might be produced. This idea, I am still disposed to entertain, and even to extend to all similar cases. And the conclusion is the more probable, in relation to the like events in this country, in as much as thunder storms are of rare occurrence, and seldom severe excepting in summer and autumn, and in the most sultry weather, when the tendency to rapid putrefaction is greatest.

It is mentioned in the account of the *post mortem* appearances, that the fingers were contracted and rigid. Hunter was of opinion, that the muscles of those struck by lightning are relaxed and entirely deprived of their tone. I am equally ignorant how he arrived at this conclusion, which I am disposed to think was peculiar to him, and, perhaps, founded on hypothetical views. Some of the older writers, particularly insist on the rigidity of the muscles in those struck by lightning; and Mayow, in his *Tractatus Quartus de Motu Musculari*, endeavours to explain, by means of his very original speculations connected with the nitro-aërial principle, “*cur ful-*

mine percussi obrigescunt," treating of the effect as if it was then generally acknowledged.*

4.—*Effect of Hydrocyanic Acid, and of violent and long-continued Muscular Exertion on the Blood.*

It has been asserted that the blood of animals, hunted to death, and killed by the prussic or hydrocyanic acid, continues in the liquid state; and even the high authority of Hunter may be adduced in support of the opinion, that, in animals run down in the chase, the blood is deprived of its power of coagulating. From all the information, however, which I have been able to collect, I am compelled to conclude, that the opinion in both cases has been hastily taken up, and is not correct.

In relation to the hydrocyanic acid, in no instance have I witnessed the blood liquid, or deprived of its power of coagulating, in animals poisoned by it,—and I have paid attention to the state of the blood in many animals thus killed,—the cat, the dog, the rabbit,—and the appearance which it has exhibited, has been in no wise peculiar; its coagulation, as well as I could judge, was neither retarded nor accelerated. And this might be fully expected *à priori*, considering that the hydrocyanic acid has no visible effect, even in large quantity, when mixed with the blood, out of the body.

Relative to animals hunted to death, the results of

* Op. cit. Cap. vi. ad finem.

my inquiries has been very similar. From what I have been able to learn from sportsmen, and especially from some of my professional friends, familiar with field-sports, coagulated blood is commonly to be found in the hearts of animals thus killed; and I am authorized by an observer, on whose accuracy I have perfect reliance, viz. Dr. Andrew Smith, to state, that this is the result of his experience in Southern Africa, where the larger antelopes are frequently run down by dogs. Perhaps, in this instance, as well as in that of death from lightning, when a loose or liquid state of the blood has been observed, putrefaction has been more concerned in producing it, than the mode of death.

5.—*On the Effect of Change of Temperature on the Coagulation of the Blood.*

It is agreed by all those who have made experiments on the subject, that cold, instead of being the cause of the coagulation of the blood, as was for a long time believed after Aristotle,* actually retards

* Aristotle, speaking of the coagulation of the blood, in connexion with its cause, says, “Si fibras detraxeris, sanguis non cogetur. Ut enim si ex luto terrenam portionem semoveris, aqua non concrecit; ita sanguis, fibris detractis, incongelabilis manet: fibrarum enim terrena est. Quod si non eximantur, cogitur, ut terra humida, frigore. Cum enim calor exprimitur, humor circa trahitur, atque evaporatur: atque ita concrecit, non à calore siccescens, sed à frigore.”—De Part. Animal. lib. ii. ch. 4.

Harvey, as appears from the passage already quoted, refers the coagulation of the blood, to an escape of the native animal heat; and Sydenham to the cold of the atmosphere.

coagulation. My own results are in conformity with this, and with the results of the accurate Hewson, that the viscosity of the blood is, at the same time, increased. At the temperature of 32° , I have seen blood remain liquid more than an hour; a reduction of temperature, some degrees lower, was required to freeze it,—when it appeared as a homogeneous mass.

Hewson, from his experiments on the freezing of the blood, inferred, that its qualities were not thus altered; that when thawed, it returned to its liquid state, and that it was then subject to coagulate. He speaks very decidedly on this point: he says,—“the blood was always evidently fluid on being thawed,—and as evidently jellied when exposed to the air.”*

Mr. Thackrah, from his experiments, infers the contrary, viz.—that by freezing, the blood permanently loses its power of coagulating.†

The few experiments which I have made on this subject, instituted long before I had seen Mr. Thackrah's book, perfectly agree in their results with those of Mr. Hewson. I shall describe them from notes taken at the time, in the month of January, 1828, at Corfu.

Received into a thin glass tube, the temperature of which had been reduced by ice and dilute sulphuric acid, some blood of a fowl, and immediately

* Experimental Enquiry, p. 28.

† An Inquiry into the Nature and Properties of the Blood, by C. T. Thackrah, 2nd Edit., London, 1834, pp. 67, and 37.

placed it in the freezing mixture. The blood was frozen in a few seconds. After three or four minutes it was taken out of the mixture; it presently became liquid. It was again replaced and refrozen. It was once more taken out, and it melted again; and it was again speedily replaced. At the end of four or five hours, left in ice thawing, and which was then thawed, I found it coagulated. Some of the same blood, not cooled artificially, coagulated in less than a minute; and in less than three minutes, when received into a platina crucible, and suddenly cooled by ice, to 40° .

The subject of the next experiment was venous blood, from a soldier labouring under a slight ailment. About an ounce of this blood, received into a glass tube, was instantly put into a freezing mixture. After about ten minutes, a thermometer introduced into the blood fell to 26° . It remained at this temperature about an hour, and all the time liquid: the red particles had subsided, and the supernatant fluid was colourless. It was then put into a freezing mixture of greater strength, and placed under the receiver of an air-pump; the air was exhausted, and the pump was worked till the blood was apparently frozen. After it had been some minutes frozen, I took it out, and applying a thermometer, found the freezing mixture at 10° . On pressing the frozen white part, a little fluid rose from the cruor,—the temperature of which was found to be 32° . The experiment was repeated till the whole was frozen. The lymph was

kept in a frozen state about twenty minutes,—the cruor about three or four. The blood was now placed in a room, the temperature of which was about 55°. It gradually thawed, and, when thawed, was liquid,—perhaps a little thicker than when first drawn. From the time it was completely melted, it began to coagulate in about eight minutes,—and, in about ten minutes, a soft crassamentum was formed. On examination, the following day, the crassamentum was found soft and not much contracted; serum had separated from it distinctly.

These results sufficiently accord with the more numerous experiments of Hewson, and in consequence, I am more disposed to consider his inference just, than the contrary one of Mr. Thackrah. And, if we examine the detail of the experiments of this ingenious inquirer, this conclusion, I think, will be strengthened. In one experiment, after having been thawed, he states that a small quantity of serum separated from the blood; in another, when the thawed blood was mixed with water and passed through a filter, some very small portions of coagulum were retained; and describing his results generally, he says, that the blood “on being afterwards thawed, becomes a grumous mass, and does not separate into serum and crassamentum.” Now, the grumous mass, and the other particulars mentioned, indicate at least a certain degree of coagulation. As in one experiment, he appears to have placed the blood to thaw in a warm

room at 65° , and to have observed it eighteen hours after, it is not improbable, that the fibrin might have been in part reduced to the liquid state from putrefaction. Be this as it may, negative results must almost always be considered of inferior value and force than positive; and if exceptions are to be made, commonly there is more propriety in selecting, for this purpose, the former than the latter.

Admitting the accuracy of the above, it is probable, that blood may be frozen for any length of time, without losing its power of coagulating, and without losing its power of becoming liquid; a happy circumstance, in relation to the animal economy in these regions, in which the cold of winter is severe, and exposed parts of the body are liable to be frost-bitten; and perhaps equally so, I may remark farther, is the circumstance of the fluidity of the blood being diminished by cold in relation to animals which hibernate during several months of the year in a state of torpor, in which life is barely maintained.

From the experiments which I have made on the effect of a high degree of temperature, as 120° ; of a moderate degree, as 100° ; and of degrees between this and the freezing point, I am disposed to infer that the first immediately renders the blood more liquid, and accelerates its coagulation; that the second rather retards than accelerates coagulation; and that at 80° or 90° , its coagulation is less

rapid than at 120° , and more rapid than at 100° . It may appear a paradox, that heat above 100° should increase the liquidity of the blood and accelerate its coagulation ; but is it more so than that cold should render it, more thick and viscid, and yet retard or prevent coagulation ? I infer that the effect of heat is such as I have represented it, from observing blood heated to the degree mentioned, and which coagulated more rapidly than blood not so heated, covered with a buffy coat, when the cool blood, slower in coagulating, was free from it. The blood is a mysterious fluid ; nothing can with propriety be taken for granted respecting it. Its properties can be ascertained only by exact experiment and unbiassed observation. Every property which it possesses seems to be adapted to admirable ends ; and even these peculiarities in relation to the effect of the higher as well as the lower degrees of temperature in its coagulation, may be considered as such adaptations. The effect of high temperature on coagulation may tend to limit the destructive effect of heat to the surface ; the effect of a temperature of 80° or 90° , which is about the temperature of the skin, may tend to suppress hemorrhage, the consequence of superficial wounds.

6.—*Of the Effect of different kinds of Vessels on the Coagulation of the Blood.*

It is generally supposed, that the kind of vessel in which the blood is received, its size, form, and quality, have a considerable influence in promoting or retarding its coagulation and the formation of a buffy coat. I have made many experiments to endeavour to ascertain the truth of this opinion; but I have not yet been able to arrive at a satisfactory conclusion. I am rather disposed to think that wood and polished metals retard coagulation, and that glass and earthenware accelerate it. The form and size of the vessel will, of course, have some influence, were it merely in connexion with temperature. The remarks which I have made relative to the difficulty of ascertaining the effect of moderate motion on the blood, are particularly applicable here; and, *a fortiori*, as the effect of the quality of the vessels I have tried, is even more ambiguous than that of motion. In illustration, I shall give the results of one trial on the same blood, with different vessels, extracted from my note-book.

On the 12th December, in the Military Hospital at Corfu, blood from the arm of a patient, slightly indisposed, was received, first, into a wooden cup; second, into a circular pewter one, divided equally into two compartments; third, into a silver dish, placed on lint moistened with ether; fourth, into a small cupping-glass. The coagulation of the blood

first took place in the wooden dish, and not at the circumference, but in the *middle* ; next, in the semi-circular compartment of the pewter vessel, in that in which the quantity received was greatest, and in it also in the middle ; next in the silver dish at the surface ; and, last, in the other compartment of the pewter vessel, in which only a very small quantity, about four or five drops, had been spilt.

Perhaps it may safely be laid down as a general rule, that the form and quality of vessel best adapted for preventing sudden cooling, and therefore an increase of the viscosity of the blood or coagulable lymph, favour most the rapid subsidence of the red particles, and consequently the formation of the buffy coat. This is in accordance with Dr. Benjamin Babington's experience with thin pear-shaped glass vessels, such as he used for the purpose of ascertaining the specific gravity of the blood ;* and also with mine, on thin globular glass vessels. Very often I have found a buffy coat in these globes, when none appeared on the blood collected in the usual way.

7.—*On the Influence of the Blood-vessels or of Vital Action on the Coagulation of the Blood.*

This is allowed to be, and necessarily is, a point of inquiry of great obscurity ; both as regards the fluid itself and the causes referred to. I shall enter on it very partially.

* Medico-Chirurg. Trans. vol. xvi. 297.

Hewson, from his experiments and observations, inferred that "the properties of the blood depend on the state of the blood-vessels; or that they have a plastic power over it, so as to be able to change its properties in a very short time;" that what increases or strengthens vascular action, or the heart's action, tends to retard coagulation, and that which has a contrary agency, promotes and accelerates it.*

He arrived at this, which he calls his "most remarkable conclusion," from witnessing the sudden change in the coagulating power of the blood, during the same blood-letting, as exemplified in different portions, received separately; confirmed by what he witnessed in instances of animals bled to death.

In these last-mentioned instances, it has been shewn, that the specific gravity of the blood which flows from the slaughtered animal, when nearly exhausted, is less than of that which flows when the vessels are first divided.

The circumstance necessarily gives rise to reflection. As it seems to shew that the composition of the blood is altered at least as regards the relative proportions of its constituent parts, as obtained in the process of draining the vital fluid, it is matter for consideration, whether something similar may not take place in the other instances referred to by Hewson; and whether the changes in the circulating mass which he attributed to the vessels, acting on the blood,

* Op. cit. p. 125.

may not be owing to a vascular action more analogous to that of secretion.

It is easy to conceive that under different degrees of vital action, the nature of the current of blood, either in the minute vessels, arterial and venous, or in the capillary vessels connecting them, or in both, may be modified; that in one condition more red particles may pass, in another a larger proportion of serum, in another of lymph. Is it not probable, that, were blood extracted from the cheek, under the influence of the peculiar feeling connected with blushing, it would be found to contain an undue portion of red particles?—that the reverse would happen, in blood abstracted from the same surface under the influence of fear; when probably it would be found to abound more in serum. And, in a third instance, as in erysipelas of the face, it would probably be found to contain an excess of fibrin.

In the embryo, a colourless fluid is the first which circulates.* Whether, independent of the lymphatics, there is an order of vessels in the adult

* This fact appears to have been discovered, if I may so say, at least three different times; by Glisson, nearly two centuries ago,—more recently by John Hunter,—and very lately again by MM. Delpech and Coste. Glisson, in his *Treatise on the Anatomy of the Liver*, published in 1564, says “*Materia prima, è qua partes fiunt, non est sanguis, sed succus quidam liquidus, albumini ovorum non absimilis, unde pullus efformatur.*”—*Anatomia Hepatis*, p. 495. Amstel. 1659. Needham, a disciple of Glisson, is even more explicit on the subject: in his treatise “*De Formato Fœtu*,” he speaks of a nutritive fluid, which “*per venarum oscula nondum rubentia facile recipiatur.*” p. 83.

conveying only a similar fluid, is a subject for inquiry. In the brain, in an instance of jaundice, I have witnessed, on making sections of the organ, points distinct from the ordinary blood-specks, tinged by bile, otherwise colourless, and which, were it not for the adventitious hue, would have been imperceptible. I regret I had not an opportunity of examining the fluid of these puncta with the microscope.* The most probable inference seems to me to be, that they were either divided lymphatics, or vessels of the order just alluded to. But, even supposing that no such vessels exist, the lymphatics, under different influences, may pour more or less of their contents into the general circulation, and so affect the quality of the blood.

The quality of blood abstracted by the leech perhaps may be adduced in support of the argument. It is commonly believed, that the blood so abstracted, does not differ from that procured by the use of the lancet. According to my observations, it abounds much more in red particles, and very much less in serum and lymph; and in consequence, its character is different,—coagulating much less firmly, or often not at all, exhibiting merely a grumous appearance, of unusually high specific gravity, and when evaporated to dryness affording a larger

* Since the above was written, another similar opportunity has occurred; the yellow fluid of the puncta, under the microscope was found to contain a very few blood-particles: it was probably, therefore, extremely dilute blood.

proportion than common of solid matter. One leech, which I tried in Malta, in May 1830, gorged with blood, was of specific gravity 1087; cut into two, and deprived of blood, one moiety was of specific gravity 1062; the other of specific gravity 1054; the latter was the caudal portion. This leech, distended with blood, weighed 161·4 grs. Another, smaller, which distended with blood weighed only 18·4 grs. was with the blood of specific gravity 1076, and empty, or very nearly so, of specific gravity 1051. Blood abstracted from the arm, from a patient labouring under an inflammatory complaint, of specific gravity 1061, and which after coagulation was slightly buffed, yielded on evaporation to perfect dryness 20·7 per cent. solid matter. Blood abstracted from the same patient from the scrobiculus cordis by a large leech, similarly evaporated, yielded 22·3 per cent. Now is it not probable, that these peculiarities of the blood abstracted by leeches is connected with the size of the aperture, and the effects of the pumping force exercised by the animal? That the latter influence is concerned, at least in part, may I think be inferred from the circumstance, that the blood which flows from the wound after the removal of the leech, commonly coagulates more readily and firmly than the blood procured from the leech itself. The blood extracted by the mosquito, is again different in character from that by the leech. It coagulates readily, and is not deficient in fibrin.

8.—*On the Effect of Oxygen and of Carbonic Acid Gas on the Blood.*

My brother, the late Sir Humphry Davy, in his *Researches on Nitrous Oxide*, states that he did not find the coagulation of venous blood affected by either of these gasses.* According to Sir Charles Scudamore, blood coagulates sooner in oxygen gas than in atmospheric air, and cools slower; and, in carbonic acid gas it coagulates slower, and cools sooner.† The experiments which I have made have afforded results similar to those first mentioned. I have not found the coagulation of the blood distinctly accelerated by oxygen or retarded by carbonic acid; nor have I found any well-marked difference in either, in relation to the time of cooling. And, I did not simply expose the *surface* of the blood to the influence of the gasses, in the manner in which Sir Charles Scudamore made his experiments, but I agitated the blood in each gas. Of the oxygen, there did not appear to be any absorption; if there were any, it must have been slight, which the form of the experiment was not fitted to detect. Of the carbonic acid gas there was a very considerable absorption, equal in volume to that of the blood used, or very nearly so. I have also tried the effect of water strongly impregnated with carbonic acid gas on the blood, and of oxygen gas on the crassa-

* *Op. cit.* p. 381.

† *Essay on the Blood*, pp. 59. 61.

mentum of the blood, and the results have been similar. The temperature of the cold crassamentum was not raised, and the coagulation of the fluid blood was not prevented.

Merely hypothetically considered, I do not comprehend how either oxygen, hydrogen, or azote, or any other gas which is not absorbed rapidly by the blood, can materially affect the coagulation of this fluid, or can be the means of producing a sensible evolution of caloric, at least by the simple contact of surfaces. I would remark, however, that I do not consider the negative results of my experiments with oxygen in the slightest degree hostile to that view of the generation of animal heat, in which the chief source of it is referred to the lungs. I need not say, that there is an immeasurable difference between the natural process of respiration and the rude experiments just now alluded to, whether we consider the apparatus, the material, or the situation, and more particularly when we consider that in one instance vitality is present, and in the other that life is extinct, and the agents are merely chemical and dead.

I must confess too, that, viewed hypothetically, I am equally at a loss to conceive how carbonic acid gas should retard the coagulation of the blood. If it were true, that blood abounds in carbonic acid gas, and the coagulation of the blood depends on the evolution of carbonic acid gas, then in the act of coagulating, it ought not to absorb an additional

quantity of this acid gas ; or if it did absorb it, it ought not to coagulate ; and indeed it ought not to coagulate, though it did not absorb it, so long as it was in an atmosphere of carbonic acid gas and so long as it retained the proportion of gas supposed to belong to it. Were there not a consistency in nature, there could be no science. Is it not a contradiction, that the blood, a fluid not saturated with carbonic acid, capable of combining with a fresh portion, should contain free carbonic acid, and be capable of giving it off. I speak of the blood out of the body, as of a bone or muscle, detached from the animal, deprived of life, and subject to the same laws as ordinary dead matter. Farther on, I shall recur to the important points,—the carbonic acid in the blood, and the absorption of oxygen by the blood, with which are involved so much of physiological doctrine.

9.—*On the Effect of removing Atmospheric Pressure from the Blood, and of excluding Atmospheric Air.*

According to Sir Charles Scudamore, blood coagulates in vacuo more rapidly than in the open air, and becomes of a considerably darker colour.* I have tried this experiment repeatedly, and the result apparently has been negative. The blood which coagulated under the exhausted receiver, was of the

* Op. cit. p. 22.

same colour as some, which for the sake of comparison, was left exposed to the atmosphere, and its rate of coagulating was much the same—certainly not quicker—rather slower. On account of the interest of the subject, I shall describe two or three experiments, from which, and from others, I made these deductions in January 1828, at Corfu.

Put under the receiver of an air-pump about an ounce of venous blood, collected as it flowed from the arm in a cylindrical glass vessel, and immediately exhausted the receiver. Until the exhaustion was nearly complete the blood was perfectly tranquil; suddenly it boiled up, almost in the manner of an explosion, and continued bubbling during half a minute at least. On the admission of air into the receiver, all the bubbles instantly subsided. The blood was not coagulated; it coagulated slower than a portion set by in the open air for comparison.

A few days after, I repeated this experiment, with this difference,—the air of the receiver was slowly exhausted to allow the blood to cool below the boiling point of water in vacuo. Now, there was no ebullition, no effervescence, no disengagement of air, excepting of two or three adhering globules. When taken out, this portion of blood had just begun to coagulate; another portion, left exposed to the atmosphere, was coagulated firmly. The colour of each was similar.

Repeated the experiment; taking precautions to prevent, as much as possible, the adhesion of air to

the sides of the glass, by careful cleaning, and wiping, and rinsing it out, previous to the admission of the blood, with distilled water, which had been purged of air. In this instance, no bubbles of gas appeared in the blood on exhaustion, and the coagulation took place a little later than in another portion of blood exposed to the air. There was no difference of colour between them.

In the paper in which the general results of these experiments were given, and which was published in the *Edinburgh Medical and Surgical Journal*, vol. xxx, for 1828,—I further stated that I found no material effect produced on the coagulation of the blood, by receiving it as it flowed from the arm into oil, which had been previously deprived of air by the air-pump, whether under the influence of atmospheric pressure in the open air, or excluded from it, under an exhausted receiver.

Turning back to the notes of these experiments, I find it remarked, that the blood coagulated slower under the oil (whether under pressure or not) than when exposed to the air;—and that, in the former, a much thicker buffy coat formed, than on the latter. This result accords with others which I had obtained several years previously in Ceylon; and also with the results of the later experiments of Dr. Benjamin Babington, on the effects of oil.* Probably it is, in part, connected with the cooling effects of the

* *Med.-Chirurg. Trans.* xvi. 298.

oil, on the blood, in passing through it; and, in part, to the exclusion of air.

10.—*On the Effect of Water, Milk, Urine, and Bile, on the Coagulation of the Blood.*

It has been remarked by Dr. Crawford, in his treatise on Animal Heat,* that, by the dilution of blood with water, in the proportion of twelve parts of the latter to one of the former, its coagulation is retarded for several hours. In one instance, in which I made this experiment, at the end of two hours, the diluted blood appeared to be liquid; but, when thrown upon a filter this appearance proved to be deceptive; the fibrin had coagulated, and it was collected on the filter in the form of a very tender gelatinous mass. From this, and from other experiments, I am inclined to infer, as might be expected *à priori*, that dilution, by removing the particles of fibrin, to a greater distance from each other,—and by suddenly cooling the blood, does retard its coagulation,—but not so long as Dr. Crawford conceived, who formed his opinion on the subject merely from the appearance of the blood.

Milk, urine, and bile, I find, also retard the coagulation of the blood, and more so even than water. The urine and bile used were human,—the milk, cow's.

* Experiments and Observations on Animal Heat, p. 278, 2nd edition.

The results of the experiment with the urine, are, perhaps, not undeserving of more particular notice. About two ounces of blood were mixed as it flowed from the arm, with four ounces of urine, containing, perhaps, rather a larger proportion than usual of urea. After about an hour and half the blood had gelatinized. After twenty-four hours, it had coagulated pretty firmly; the coagulum was much contracted and slightly cupped, and covered with a thick buffy coat. After forty-eight hours the coagulum was still more contracted, and it had acquired a globular form with a little concavity on its upper surface. The urine was only just perceptibly coloured by the colouring matter of the blood. I may here incidentally remark, that this fluid is particularly well adapted as a medium for displaying the phenomena of the coagulation of the blood, from its retarding the effect, and yet not diminishing it,—and from its not dissolving the colouring matter of the red particles. It enables one to determine, in a satisfactory manner, especially with the aid of sections, that the phenomena of the buffy coat, and the cupping of the blood, are owing to an unequal distribution of the fibrin; and that, when they occur, there is an excess of fibrin at and towards the surface of the crassamentum, which, it may be inferred, was pressed up by the red particles in descending; and a deficiency of it at the bottom, and towards it, in regular gradation; and that the buffy coat is cupped in conse-

quence of the unusual contraction of the fibrin and its accumulation at the circumference.

11.—*On the Coagulation of Serum.*

It has been supposed by Mr. Brande, and after him, it has been asserted by several authors, that the liquidity of the serum of the blood, is owing to the presence of an alkali,—and its coagulation, under certain circumstances, to the abstraction or neutralization of this alkali.* Dr. Bostock has objected

* Whether the property of serum was known to the ancients, or, indeed, whether they were acquainted with the fluid itself, is, by some, thought doubtful, so imperfect is the history of physical science; moreover, it is even doubtful at what time, and by whom, the coagulability of serum, by heat, was first ascertained. Some writers, as Haller, refer, if not the discovery, at least the first mention of this quality to Harvey;—others, as Dr. Bostock, consider Harvey's notice of it obscure,—and seem to be of opinion that Lower was the first who clearly described it. Lower, in his account of the *liquor pericardii*, remarks,—“Porro humorem istum non merè excrementitium aut instar roris stillatitii aqueum, sed seri potius nutritii è sanguine promanantis, partem esse, exinde constat, quòd ignis calori vel paululum admotus, non aliter quàm serum sanguini post venæ sectionem innatans, aut lymphæ à glandulis secreta in gelatinam albam incrassatur: qualem quidem consistentiam nec sudor neque urina qualicunque coctione acquirit, sed vel omnino in auras exhalat, aut in sabulum induratur”—(Tract. de Corde, 1669, p. 6.) The author here speaks of the property as if well known to belong to the serum of the blood; and, I apprehend, the same inference may be made from certain remarks of Harvey, in connexion with coagulable lymph,—his *mucago* “si saniem illam a grumo separatam et effusam leni foco decoxeris, eandem brevi in mucaginem hanc mutatum iri conspicias. Indicio manifesto, aquam illam sive saniem, a sanguine

to this view, on the ground of the minute portion of alkali existing in the blood,—and that this alkali may be neutralized,—and yet the serum remain liquid. He is of opinion, that the effect of coagulation, in the instance of serum, is one *sui generis*.*

The experiments which I have made on serum, have afforded results in favour of this latter view. I shall briefly describe them,—preliminary to others on the coagulation of the blood.

The following acids, I find, do not coagulate serum, when cold, namely,—distilled vinegar, strong acetic acid, citric acid, boracic acid; the tartaric and oxalic acids, and the aqueous solution of sulphurous acid. I shall notice, in detail, a few of the experiments.

In two instances of the trial of citric acid, it was added in moderate excess to the serum, its crystals having been previously reduced to the state of fine powder. On agitation, the acid was dissolved, and

in pelvi separatam, urinæ forsan materiam aliquam, non autem urinam ipsam esse; licet colore et consistentia talis videatur. Quippe urina coctione non densatur in fibrosam mucaginem, sed potius in lixivium: aquosa autem sive saniosa hæc pars, aliquandiu leviter cocta, in mucaginem innatantem abit: quemadmodum, vice versa, mucago isthæc, corruptione recrudescent, in saniam colliquatione revertitur.”—Op. Omn. p. 406. How slow was the progress of animal chemistry, before it was methodically cultivated! Between the time of Aristotle and Harvey, no positive advance appears to have been made; and the former, probably, was acquainted with the property of serum in question; it may be inferred from some expressions which he uses.—Phil. Trans. for 1809, p. 373.

* Elements of Physiol. vol. i. p. 476.

some carbonic acid gas was disengaged. The serum acquired a slightly opalescent hue, and its fluidity, perhaps, was in a slight degree diminished. The serum used was that of the blood of the sheep, which does not coagulate at 160° .* It was immersed in a glass tube in water of this temperature; it remained transparent, and acquired the consistence of thin jelly.

The same acid was next tried in great excess,—more was added to the serum than it was capable of dissolving. The results were similar, with this difference,—that the jellying of the serum was prevented by heat,—even at the boiling temperature of water. The effects of the oxalic and tartaric acids were analogous.

Strong acetic acid, in great excess, added to the same serum, did not coagulate it,—some gas was disengaged, and, perhaps, the fluidity of the serum was slightly impaired, but of this I was not quite certain. The mixture, placed in water of 160° , firmly coagulated, and the coagulum was opaque.†

A solution, in water, of the sulphurous acid, of moderate strength, appeared to have no effect mixed

* The point of coagulation of the serum of the blood of different animals is not the same; even amongst the mammalia I have observed a variation of temperature of 10° in this respect. The serum, the subject of the experiment, coagulated at 168° .

† Vinegar, as it is well known, coagulates milk. The effect of a few drops of strong acetic acid, is immediate, even unassisted by heat, presenting a remarkable contrast with the effect of the other vegetable acids mentioned above, which, even when in large excess, and boiled, did not coagulate milk.

with serum. When, however, the acid gas was condensed in serum, there was a decided effect, a coagulation was occasioned,—a transparent jelly formed,—which appeared to be quite insoluble in water, and not liable to putrefy.

The effects of the mineral acids,—the muriatic, nitric, and sulphuric, vary with their degree of solution. Equal parts of muriatic acid and water, coagulate serum as readily as the strong acid; but, when diluted with six parts of water, the effect was scarcely visible. Nitric acid, largely diluted, has a distinct effect; thus, one part of this acid to thirty parts of water, had rather more visible effect, than the muriatic with six of water; with twenty of water the serum was rendered very turbid.

The alkaline earths, lime and magnesia, have, apparently, no effect on serum. Baryta, mixed with serum, formed, with it, a firm semi-transparent jelly. A few hours were required for the combination to take place. In the instance of lime and magnesia, no change was perceived either immediately or after many hours.

Of the salts, some, as the sulphate of copper, the sulphate of zinc, coagulated serum, or occasioned a precipitation of its albumen, and even when extremely diluted. The super-tartrate of potash, in solution, rendered it turbid. Others, as nitrate of barytes, oxalate of ammonia, borax, triple prussiate of potash, chlorate of potash,—the neutral salts, did not disturb its transparency.

Nitrate of silver unites with the albumen of the serum, which it precipitates in the form of an insoluble compound; but, though insoluble in water, it is readily soluble in acetic acid; and, it is not a little remarkable, that its solution in this acid is not precipitated by common salt.

A strong aqueous solution of chlorine did not coagulate serum.

Calf's rennet was tried on it, both cold and warm, without effect.

12.—*On the Effect of Substances on the Blood, which do not coagulate Serum.*

According to their effects, as I have ascertained by experiment, these substances may be divided into three classes; viz. 1st, those which do not retard distinctly, or distinctly accelerate the coagulation of the blood; 2d, those which retard or prevent its coagulation; and 3d, those which accelerate its coagulation, or otherwise alter it.

(1.)—*Of those substances which do not distinctly retard or accelerate coagulation.*

The following substances, which I have tried, may be enumerated as belonging to this class, viz. chloride of silver, protoxide and peroxide of antimony, red precipitate, sulphur, iodine, hydrocyanic acid, carbonate of lime, starch, sugar, camphor, sulphuric æther, oil of peppermint, cantharides, oil of turpen-

tine, hydroboracic acid, white oxide of arsenic, and gum-arabic.

In each instance, there was some slight peculiarity in the result, as in relation to the colour which the crassamentum exhibited, or its degree of contraction or consistency. I shall notice a few of them, those which were best marked.

Sugar, refined in powder, added to the blood the instant it was drawn, did not appear to retard coagulation; the coagulum was very soft, and bright red. After twenty-four hours very little serum had separated, and there was scarcely any perceptible contraction of the clot; the fibrin collected by means of a linen filter, gentle pressure being used, was very soft. The fluid part, obtained by filtration, did not jelly when diluted with water. The experiment was made in Corfu, in November, when the temperature of the air of the room was between 50° and 60° . After a week, there were no signs of putrefaction, either in the diluted or undiluted mixture; both had a very slight smell of vinegar.

The effect of the white oxide of arsenic on the blood differed but little from that of sugar; it did not appear either to retard or accelerate its coagulation. The coagulum, however, after twenty-four hours was comparatively soft, having scarcely perceptibly contracted, and no separation of serum had taken place. The surface was unusually fluid, and also the bottom of the coagulum in contact with the principal part of the oxide. This experiment was

made in England, in January, on the blood of a young man, which was healthy in appearance. It appeared to prevent putrefaction.

Gum-arabic, added in the state of fine powder, did not appear to retard the coagulation of blood, excepting, perhaps, in a very slight degree. The coagulum formed was very soft, and it contracted very little. Mucilage of gum-arabic, retarded it about twenty-four hours.

Oil of turpentine, also, perhaps retarded the coagulation of the blood in a very slight degree. The effect on the colour of the blood was more distinct; the oil changed it to a brownish hue.

Cantharides in powder did not appear to retard coagulation; the crassamentum remained soft, and not contracted.

Red precipitate neither retarded coagulation, or the contraction of the crassamentum when formed, nor consequently the separation of the serum.

Iodine did not appear to retard or accelerate coagulation. Eight grains of it were added to an ounce and half of blood. It rendered the blood brownish.

(2.)—*Of those substances which retard or prevent coagulation.*

The following substances, which I have tried, belong to this class; viz. rhubarb, ipecacuanha, cinchona, columba, myrrh, catechu, jalap, the extracts prepared *in vacuo* of belladonna, digitalis, aconite, conium

and sarsaparilla, the common extract of cinchona, magnesia, carbonate of magnesia, sulphate of zinc, tartarized antimony, supertartrate of potash, triple prussiate of potash, muriate of barytes, nitrate of barytes, borax, common salt, nitre, sal-ammoniac, the sesquicarbonate of ammonia, the carbonate of soda, sulphate of ammonia, aqua ammoniæ, aqua potassæ, distilled vinegar.

The effects of the substances arranged under this second class, individually considered, are still more various than those of the first. Most of them produced a thickening, and increased the viscosity of the blood. The former was especially the effect of the vegetable substances, and the latter of some of the salts, particularly borax and carbonate of soda. Very few of the substances tried had a contrary effect; it was well marked only in a few instances, as in those of the triple prussiate of potash, cream of tartar, tartarized antimony, magnesia, and carbonate of magnesia, with each of which the red particles rapidly subsided; no coagulation took place, and the fibrin remained completely dissolved in the serum. In some instances, and that the majority, owing to the thickening of the blood, or the increased viscosity imparted, the red particles were kept suspended. In a very few, as in those of distilled vinegar, aqua ammoniæ, and aqua potassæ, they were dissolved. The difference of effect, as to change of colour, was very various indeed; most commonly the colour was darkened,—in a very few instances

it was rendered of a brighter and lighter red. This latter effect was most distinct in the instances of nitre, common salt, and borax; it was never permanent. On dilution with water, almost in every instance, the influence of the foreign substance ceased, and the blood coagulated; and, I believe, it would have done so in every instance, with the exception of vinegar, and aqua ammoniæ, and potassæ, and apparently of magnesia, were the due proportion of water known which should have been added. Mr. Hewson, I believe, was the first who noticed this suspension of the power of coagulation and the restoration of it by the addition of water. His observations were confined to the neutral salts.* The notices which he gives of his experiments are very brief; he does not allude to the length of time the effect of suspension of coagulation may be continued, nor does he appear to have been aware, that conjointly with this the tendency to putrefaction is arrested. From what I have witnessed, I believe

* The following note appears to contain the results of Hewson's experiments on this subject,—“The salts which keep the blood fluid by itself, and yet allow it afterwards to jelly on being mixed with water, are sal glauberi verus (sulphate of soda), sal digestivus sylvii (chloride of potassium), sal communis, nitrum commune, nitrum culicum (nitrate of soda), sal diureticus (acetate of potassa), borax, the salt made of vinegar and the fossile alkali, and the salt made with vinegar and chalk.” He adds, “the following salts, likewise, keep the blood fluid, but do not allow it to jelly when mixed with water, tartarus vitriolatus (sulphate of potash), sal epsomensis, sal ammoniacus communis, sal ammoniacus nitrosus, sal rupillensis (tartrate of potassa and soda).”—Experimental Enquiry, foot-note, p. 13.

that blood may be kept liquid or viscid for days, or even weeks, without losing the power of coagulating, and the fibrin of contracting, when properly diluted with water. In the one state, I find it resists putrefaction ; in the other it readily yields to it. I have said, that vinegar, and ammonia, and aqua potassæ are exceptions in this class of substances, differing from them in preventing the coagulation of the blood, however carefully diluted with water, and that magnesia is apparently an exception also. The vinegar and the aqua ammoniæ, and potassæ clearly keep liquid, and that permanently, the fibrin of the blood, as well as dissolve the colouring matter of its red particles.* The appearance of magnesia being an exception also, I have found is a deception, and chiefly in consequence of the circumstance, that on dilution the coagulable lymph does not form a glutinous mass, but is precipitated in flocculi.

From these general remarks, I shall proceed now to particular instances, noticing only those in which the effects were best witnessed.

* Strong acetic acid renders blood dark and viscid; in one instance in which a drachm of acid was added to three drachms of calf's blood, the mixture, though viscid, admitted of being poured from one vessel into another. The red particles did not subside; under the microscope, their appearance was distinct, perhaps a little wrinkled at their edges, and perhaps a little expanded. When a drop of this viscid blood was mixed with a drachm of strong acid, from almost black it became of a reddish hue; and the red particles, it may be inferred, were dissolved, as under the microscope they were no longer perceptible.

Extract of digitalis prepared *in vacuo*. Previous to mixing it with blood, it was softened with water and made of thin consistence; a dram of water having been added to half that quantity of extract. The (blood about two ounces) as it flowed from the arm, was received into the vessel containing the extract and immediately stirred. The first effect was a thickening of the blood; it acquired the consistence of paste. After twenty-four hours, the appearance was much the same, as it was also after thirty-eight. The following day, the lower part of the mixture was found to be more viscid, as from the subsidence of the coagulable lymph. Water was now mixed with it, and the result was that it jellied.

Extract of opium; similarly and in like quantity mixed with water, and afterwards with blood. After two hours the blood was liquid; the red particles had subsided, leaving a stratum of viscid fluid which admitted of being drawn out into threads; after three hours, slight coagulation had taken place; after twenty-four hours, the coagulation was firm, cupped, and exhibited a buffy coat.

Sulphate of zinc. A drachm of this salt in powder, was mixed with about an ounce of blood. It thickened immediately but did not coagulate. After twenty-four hours there was no further apparent change. On dilution with water, a white pellicle formed suddenly, resembling a false membrane, from the sudden coagulation of the fibrin.

Sulphate of ammonia, in aqueous solution. After

three hours the blood with which it was mixed was liquid and its colour unchanged. After twenty-four hours, the colouring matter had subsided, the supernatant fluid was like serum, it did not jelly.

Magnesia and carbonate of magnesia—15 grains of each in impalpable powder were mixed with half an ounce of blood. The blood in each instance acquired a more florid hue, remained liquid, and the red particles subsided. On the following day, viz. the 19th January at Corfu, the supernatant fluid, had the appearance of serum. Now, diluted with water, after twenty-four hours, in the instance of the carbonate it was found to have jellied; and in that of the magnesia, to have afforded a precipitate of lymph in a flaky form.

Cream of tartar: a drachm of this salt was added to an ounce of blood. The blood thickened a little and effervesced slightly, and became of a brighter red. After three hours it had not coagulated. After twenty-four hours the red particles had subsided; the supernatant fluid was like serum. On dilution with water it jellied; the coagulum was of a very soft consistence.

Chloride of barium: 12 grains of this salt were mixed with an ounce of blood. After five minutes, it remained liquid; the colour of the blood was perhaps a little brighter. After twenty-four hours, there was a slight subsidence of red particles and separation of fluid. It was a little thicker, but still liquid. With water it jellied.

Nitrate of barytes, mixed with blood in the same quantity, had an effect very similar.

Muriate of ammonia: 20 grains of this salt in powder were mixed with an ounce of blood. The blood was immediately darkened. After twenty-four hours, it was of a very dark-red, and viscid like thick bile. No fibrin could be obtained separate, by passing it through a linen filter. It had a strong ammoniacal odour. A portion of it diluted with water coagulated pretty firmly. After six days, in November, at Corfu, the diluted blood became putrid. The portion not diluted was free from putridity, and after this interval coagulated with water as in the first instance.

Sesquicarbonate of ammonia: 50 grains of this salt in powder were mixed with three ounces and a half of blood. There was a change of colour as in the preceding, in which the muriate was added. After twenty-four hours, it had separated into two parts, a supernatant orange-yellow fluid, and a viscid dark-red sediment. A portion of each diluted with water coagulated; the former firmest. After three days, in the same place and season, the diluted portion was slightly putrid; whilst that not diluted was unchanged, and retained its power of coagulating when mixed with water.

Sesquicarbonate of soda: 50 grains of this salt in powder were mixed with three ounces and a half of blood. The blood immediately became of a brighter red. After twenty-four hours it darkened,

became very dark and viscid, in consistence like thick bile. There was only a very slight subsidence of the red particles. On dilution with water, it did not coagulate. The portion diluted after three days, in November, was putrid: the undiluted portion was free from taint.

Common salt: two drachms of this salt in powder were mixed with two ounces of blood. Examined a few hours after, the blood was found thickened, but not distinctly coagulated. After forty-eight hours, it was much in the same state. Passed through a linen filter, no fibrin remained. Diluted with about four times its bulk of water it coagulated and formed a gelatinous mass.

In another experiment, 20 grains of common salt were mixed with an ounce of blood. It prevented coagulation: and the colour of the blood was immediately brightened. After twenty-four hours the blood was found viscid and dark. Filtered through linen, no fibrin was collected. It jellied with water. After six days, in November, the diluted portion had become putrid; the other not, and retained its power of coagulating on admixture with water.

Saltpetre: mixed two drachms of this salt with two ounces of blood; after a few hours the blood was found thickened. Passed through a linen filter, a minute portion of very soft fibrin was collected; after forty-eight hours, the filtered portion jellied with water.

In another experiment, 20 grains of this salt

were mixed with an ounce of blood; partial coagulation presently took place; after twenty-four hours, a soft crassamentum formed, and pretty much fluid separated. Subjected to filtration, gentle pressure being used, some fibrin of soft consistence was collected. The filtered fluid coagulated weakly on dilution with water. After twenty-four hours, this portion was putrid: the undiluted had no unpleasant smell; but it did not now coagulate with water.

In a third experiment, twenty grains of nitre in powder were mixed with an ounce of blood, previously deprived of its fibrin, by agitation with pieces of lead in a bottle, quite full of blood, and from which in consequence atmospheric air was excluded. The colour of the blood became a little lighter, but rather of a brick-red hue than vermilion. Now on exposure to the air, at the surface it acquired a very florid scarlet hue, very similar to that imparted by oxygen. This thin coloured stratum subsided, and it was succeeded by another and another; till the whole mass exhibited a stratified appearance—a succession of bright and darker lines; analogous to what takes place in the cruor, similarly exposed without the addition of nitre, but less strongly marked.*

* These results accord with those of Haller, as described by him in his *Elementa Physiolog.* ii. p. 74. After noticing the difference of opinion previously existing on the subject, whether nitre dissolves or coagulates the blood, he says, “*A nitro sanguis neque solvitur, neque cogitur, in meis quidem experimentis. Admisto enim nitro laminae equidem cruoris floridissime rubentes nascuntur, verum eae,*

Borax : fifty grains of this salt were mixed with three ounces and half of blood ; it immediately assumed a brighter red ; after twenty-four hours, the blood was liquid ; the red particles had completely subsided ; the supernatant fluid was of a bright yellow colour, like serum. Both the inferior and the superior portion coagulated pretty firmly on dilution with water. After three days the diluted portion was slightly putrid ; the other was free from any unpleasant smell, and retained its power of coagulating ; and even after sixteen days it possessed this power. The experiment was made in Corfu, in November.

Triple prussiate of potash : a drachm of this salt, in fine powder, was mixed with three ounces and half of blood ; coagulation was prevented ; the colour of the blood was a little brightened ; the red particles rapidly subsided, and the supernatant fluid

etiam absque nitri ope, pariter ex eodem sanguine coeunt. Color autem vividissime coccineus utique nitro debetur, quo sale non alius lætore purpure sanguinem ditat.” Relative to the difference of opinion previously entertained, he observes, “*Ceterum, si de lite illa omnino definire oporteret, crederem nitrum sanguinem potius resolvere, siquidem leviores reddit, neque mea experimenta penitus repugnant.*” He adds, “*Alii sales medii utcunque similia faciunt. Sal marinus cum sanguine commistus floridum colorem facit, cæterum coagulationem neque valde promovet, neque impedit, nisi motus accesserit, qui etiam solus coagulationem impedit.*” This may be considered as a tolerable summary of what was then known on the subject, coupled with a remark which precedes his observations on nitre. “*Ea communis salium mediorum et lixiviosorum natura est, ut fluidum plerumque sanguinem reddant, aut liquidum conservent, colorem vero rubrum aut firment, aut omnino augeant.*”

was like serum. On dilution with water, the fibrin coagulated very slowly, and the coagulum which formed was of very soft consistence.

(3).—*Of those substances which accelerate coagulation, or otherwise alter the blood.*

The following substances, I find, belong to this class, namely,—the oxalic, tartaric, and citric acids, chlorate of potash, lime, and calomel. No doubt, the number will augment on farther inquiry. I shall notice, briefly, the apparent effects of each.

Oxalic acid: fifty grains of this acid, in powder, mixed with two ounces of blood, immediately coagulated it; the coagulum was of a pitch colour, and of firm consistence, and no serum separated from it. A smaller proportion of the acid rendered the blood thick.

Tartaric acid: thirty grains of this acid, dissolved in two drachms of water, mixed with two ounces of blood, prevented immediate coagulation; after two hours, the mixture became frothy from the disengagement of air, thick like tar, dark brown and viscid; after twenty-four hours it had lost its viscosity, was firmly coagulated, but without contraction and separation of serum, and was easily friable.

Citric acid: thirty grains of this acid, with the same quantity of saturated aqueous solution, mixed with two ounces of sheep's blood, slightly retarded its coagulation; in half an hour the blood coagulated. The coagulum was firm and tough, and of a brownish

hue; after twenty-four hours it had not contracted or yielded serum. A small portion of it, agitated with serum, reddened it. A portion of a drop of this reddened serum, under the microscope, was found to contain red particles, little, if at all altered;—they had their usual disc-like form.

Lime, mixed with blood, immediately darkens its colour, and speedily forms with it a tough mass, which, on exposure to the air, shortly becomes firm and hard.

Chlorate of potash: fifty grains of this salt, in powder, mixed with an ounce of blood, retarded its coagulation, and brightened its colour; after twenty-four hours, it was found firmly coagulated and of the colour of pitch. No serum had separated from the coagulum; it contained some air-bubbles and had a peculiar smell.

Calomel: thirty grains of this chloride, were added to half an ounce of blood; its coagulation was rather accelerated, and the coagulum was firm.

13.—*On the Effect of Substances on the Blood which Coagulate Serum, or Precipitate it, Albumen.*

The following substances come under the above denomination: the sulphuric, nitric, and muriatic acids; sulphurous acid, barytes, alum, nitrate of silver, sulphate of copper, alcohol.

All of these apparently coagulate, both the fibrin

and the serum of the blood. The effect is produced almost instantaneously, excepting in the instances of sulphurous acid and barytes, — and is generally attended with a change of colour of the red particles. Neither the sulphurous acid gas, nor its aqueous solution, nor barytes, has any perceptible immediate effect, except that of darkening the blood; but, in a few hours, both the acid and barytes, as might be expected from their operation on serum, render it of firm consistence like strong jelly.

The blood used in all the experiments, the results of which have been given, when not specially mentioned, was venous blood, taken chiefly from men in the prime of life, or but little beyond it, labouring under various diseases. I have not considered it necessary to specify the nature of these diseases, or the peculiarities of the blood in each instance,—the object of my inquiry being, on the present occasion, totally different from the investigation of the properties of this fluid in a morbid state. It may be proper, at the same time, to notice, that disease was not lost sight of,—and that in such experiments, in which it was desirable to use healthy blood, the blood employed was from men labouring under slight complaints, or just attacked with severe ones, and a portion of it was always set aside, undisturbed, for comparison.

Hewson states that he did not give a detailed

account of his experiments on the action of the neutral salts on the blood, because he was of opinion, that they were not applicable to medicine. Haller after passing in review the operation of a considerable number and variety of re-agents, arrived at the same conclusion. The paragraph in which he expresses this opinion, is deserving of attention in the present times ; I shall give it entire.

“Arsenicum, dubium corporis genus, sanguinem cogit* rubrumque reddit. Cæterum infinita alia cum variis succis instituta experimentis, apud cl. viros legere oportet, quos excitavi. Obiter autem in iis, quæ repetii, discas, quam parum ad medicamentorum virtutes dignoscendas utilitatis ab his experimentis possis arguere. Arsenicum enim, summum venenum, sanguinem fere ut beneficum illud nitrum mutat. Acetum inter omnia pharmaca tristissimum colorem sanguini inducit, non ideo minus innocuum. Acidi liquores et lixiviosi pariter cogunt.”†

Reflecting on the results which I have described, and proceeding on the principle of induction, the conclusion of Hewson and Haller, in the present state of our knowledge, appears to be the only legitimate one. Haller contrasted the effects of white oxide of arsenic and vinegar. Other substances might be brought forward in the same manner,—as the hydrocyanic and hydro-boracic acids ; the salts

* This effect I have not witnessed, vide p. 98 .

† Element. Phys. tom. ii. p. 83.

of barytes and strontites, and of lime and magnesia; the oxalic and the citric acids; and a large number of the vegetable extracts.

If no relation can be discovered between the operation of a substance on the blood, out of the body, and of the same substance taken internally, the conclusion above referred to is necessarily strengthened; and it seems unwise to have confidence in any hypothesis, founded on partial relations of the kind. As the subject of the blood, in all its bearings, is one of great obscurity, all speculations concerning it require to be received with much caution and examined with all possible rigor. Adopted, without due and searching scrutiny, hypothesis can hardly fail to exercise a very injurious influence; and to have the contrary tendency, if it stimulate research, and that careful inquiry, by which alone disputed points can be determined.

14.—*Some Remarks chiefly of a speculative kind relative to the Question, Is the Blood a living Fluid?*

By one train of reasoning, the conclusion, that the blood is a living fluid, seems almost demonstrated; whilst by another train of reasoning, the same appears almost doubtful.

Can it be supposed probable, or even possible, that a fluid, itself void of life, can circulate through the body imparting vital energy; deprived of which

fluid, every function is interrupted, every organ soon ceases to act, and the part, or parts, or whole, almost immediately die?

Again, is it possible to conceive, that a fluid which belongs only to animals, which is generated in them from its elements, which can exist in them only under ordinary circumstances, changing its character the instant it is abstracted, without absorbing, or evolving any thing sensible or cognizable “in quo (as Harvey forcibly and eloquently states) vegetativæ et sensitivæ operationes primo elucet: cui calor, primum et immediatum animæ instrumentum, innascitur: qui corporis animæ que commune vinculum est: et quo vehiculo anima omnibus totius corporis partibus influit.”* Is it possible to conceive that this is a dead fluid?

In opposition to this view, it may be replied; it is only a little more difficult to conceive the blood dead, than the chyle, and the chyle than the chyme. The food is admitted to be dead; yet it is grateful to the palate, and certainly not injurious to the gullet and stomach, and whole *primæ viæ*. From it chyme is formed. May not chyme, in relation to the duodenum, act the part of the food in relation to the stomach, and like the food be dead? Chyle is supposed to be derived, or formed from chyme; and, may not it act on the parts with which it comes in contact, like the food on the stomach, and the chyme on the duodenum, and be considered also

* Op. omnia, p. 398.

dead? And may not the same be said of the blood which is derived from it? In many of their properties, chyme and chyle have a certain resemblance, and chyle and blood a close resemblance. They (chyle and blood) both contain suspended in them solid particles of a definite form; both contain a spontaneously coagulable fluid, and, another fluid ingredient coagulable by heat: they differ chiefly as regards the solid particles,—in the one, spherical and colourless, or nearly so; in the other, coloured, and in the instance of the mammalia, of a disc-like form.

Descending in the scale of organization, the reasoning just employed appears even more conclusive. In certain animals, there appears to be but little difference between the chyme or alimentary fluid and the blood,—the former passing into the blood-vessels direct. And in certain other animals, there appears to be no difference,—their chyme or alimentary fluid, being what is commonly called their vital fluid, and distributed by ramifications of the alimentary canal, they having no regular vascular system.

In further prosecution of the latter train of argument, it might be maintained, that many of the reasons which are commonly assigned in favour of the blood not being dead, are applicable only to arterial blood; and so far they may be retorted to prove, that if one kind of blood is alive, the other is dead; and, consequently, that the circulation of the blood may be viewed as a perpetual miracle, in

which material particles are without cessation dying and reviving.

Farther, in prosecution of the argument, it may be said, that the blood considered chemically, however extraordinary, is not without analogy, and that some vegetable juices make a near approach to it in their proneness to change, and in the tendency of their elements to enter into new combinations. Witness the juice of the grape and most of the other saccharine vegetable juices, which at a certain temperature, like the blood, can only exist in their proper vessels and receptacles. The instant they are abstracted the process of fermentation begins, and that even in close vessels, and though oxygen be excluded, as it appears to me, I have proved by experiment. Here then, we have an instance of change as great as that which the blood undergoes, when drawn from the body, spontaneously taking place amongst the elements of dead matter. If it be said, that the vegetable juices possess vegetable life, by which they are preserved in their vessels, and that they are to be considered only as dead when expressed, and when in consequence they are liable to fermentation, another instance free from such objection may be substituted, viz. a solution of crystallized sugar, which is subject to the same change on admixture with a little leaven,—the leaven itself, it would appear, undergoing no change in the act of exciting fermentation.

The subject may be considered under many other

points of view, and arguments for and against the vitality of the blood easily adduced. Perhaps it may not be amiss to prosecute it a little farther, nor uninteresting to those who are fond of such speculative questions.

In support of the vitality of the blood, it may be urged, that one of its ingredients—fibrin, or lymph—when effused, often becomes solid in a very short time, and vascular. When vascular and organized, it may be admitted as alive, even by the casuist; and, also, when the vessels are forming in it, and the moment before that event; and so on to the period when it was effused, or prior to that when it formed a part of the circulating fluid: and, if one part of that fluid be allowed to possess life, it would be extraordinary indeed, if the other parts of it were destitute of life. By a similar mode of reasoning, from the changes which take place in the egg, from the formation of the foetus, and the influence of the spermatic fluid—(a fluid derived immediately from the blood)—the same conclusion may be drawn. How difficult to suppose a living fluid a particle derived from a dead fluid,—that that which animates should be itself destitute of life, or be furnished by one without life.

In opposition, it may be replied, organization is an effect of life; but the effect or product is not necessarily living, and a part may belong to the living body, and yet be without life. Tartar, a mixture of phosphate of lime and a little carbonate

with mucus, destitute of life, occasionally firmly encrusts the teeth. Dead bone, it has recently been found, may be united to living bone; a union may take place by a deposit of new bony matter, connecting the two, so that, were it not known that the added bone was dead, it might be inferred to be alive.* Moreover, we know, that foreign bodies have often continued enveloped in living parts, as a ball in a limb, as if they were not foreign bodies, occasioning no pain, no irritation, no morbid action. Very recently, a patient was admitted into the General Hospital, at Fort Pitt, after amputation of his forearm on account of a malignant tumour. The stump was cicatrized, but the ligatures employed in securing the vessels had not come away. About four months after the operation he died, exhausted from the effects of the malignant disease which had broken out in the arm and shoulder. I examined the ligatures; they had been perfectly preserved, enclosed in the cicatrix, as if they had been living parts, indicating demonstratively that lymph had been thrown out around, and that it had adhered to them.

The hairs, nail, cuticle, and enamel of the teeth are products from living parts, and all of them may be considered as organized; but they are not generally considered as alive, and yet how little do they differ

* Vide Transactions of Medico-Chirurgical Society of London, vol. xix, for a notice of this curious fact by Mr. Gulliver; and also the 3d Fasciculus of Drawings, selected from the Museum of Morbid Anatomy, at Fort Pitt.

in want of indication of vitality from the lens of the eye, from bone, cartilage, tendon, and even the blood-vessels themselves. It is not necessary to insinuate, if the former are dead, why not the latter? But, it might be urged rather, if the latter possess life, why not the former? Is it not as reasonable to suppose hair, or the cuticle alive, as the substance of an adhesion formed of effused coagulable lymph.

Or, taking a wider range of argument, the mystery of the subject may be dwelt on. It may be asked, is it more mysterious or inexplicable that a dead fluid should circulate in living vessels, than that dead parts should be attached to living parts? Is it more mysterious, that one kind of fluid, the spermatic, should excite an ovum to change, development, and growth, than another kind of fluid, milk or broth (both having been previously subjected to the boiling temperature) should nourish an animal, supplying new living particles to the body; in excess occasioning growth in infancy; in deficiency, decay in old age. The serum of the blood, after coagulation, apart from the clot, may be injected into the vessels of a living animal, and will circulate without deleterious effects. The cruor, after the separation and coagulation of the fibrin, after having been out of the body several hours, may also be injected into the blood-vessels, and resume its place in the circulation, without injurious effects;* if dead, acting as if alive, and

* Provided it is not from a different class, or even species, of animals; if the former, a fatal effect is rapid; if the latter, the effect is

consequently in a manner confounding the difference.

I am tempted to give another form of argument, because connected with a curious fact.

Against the vitality of the blood, it may be said, its characteristic properties may be preserved out of the body, under some circumstances most unfavourable to life. Thus by the addition of a neutral salt, it may be kept liquid, and on dilution with water, after many days it will coagulate. It may be kept liquid too by a temperature of 32° , I know not how long; and on raising the temperature, it will coagulate. Even freezing the blood, by reducing its temperature below 32° does not destroy its peculiar properties. When exposed frozen to a gradually increasing temperature, it liquefies, and the liquid presently coagulates. How, it may be asked, are these phenomena compatible with its being a living fluid? Can it retain life when frozen? If not, and yet become liquid and after that coagulate, its liquidity and coagulation are independent of life.

To these arguments, again, it may be replied, it does not necessarily follow that a neutral salt, or a temperature of 32° , or the freezing of blood deprives it of vitality.^a Certain seeds will bear a very low temperature, much below the freezing point of water, and germinate afterwards. Some parts of warm-blooded

^ainjuriously, often mortal. Vide Annales de Chimie et Physique, tom. xviii. p. 295, for the results of the researches of M. M. Prevost and Dumas on this subject.

animals may be frost-bitten, and yet not destroyed. The common medicinal leech may be frozen and kept frozen for a length of time, and yet not killed. In the winter of 1827, I had proof of this, when going, in company with my brother, the late Sir Humphry Davy, from England, through France, into Italy, by Mount Cenis. It may be proper to give some details respecting a fact, which to the best of my knowledge is new. For about a month, the cold was very severe, so much so, that some leeches which were kept inside the carriage in a bottle of water, were included in a solid mass of ice, from London nearly to Modena; and, it must be inferred, were frozen, as the temperature was often below the freezing point, and in passing the Alps, only a few degrees above zero, of Fahrenheit's scale; notwithstanding, when the ice was gradually thawed, the leeches all, with the exception of one, revived, and after this lived a considerable time.*

It may indeed be said, however improbably, all the circumstances considered, that the leeches were

* The fact is thus described by my brother, in his Journal: "On the 14th (March, 1837), I made use of two leeches, which bit immediately, and performed their office well; yet they had been frozen from January 23d to Feb. 22d, and had been sometimes exposed (as on Mount Cenis) for more than 15 hours to a temperature below 10° Fahrenheit. They were thawed very gradually, and appeared dead when first thawed, but recovered in some hours in a warm room. They had all (they were twelve) adhered together in a sort of ball, and were precisely in the centre of the bottle, at the greatest distance from the cooling causes."

merely included in the ice in this instance, and not actually frozen. To ascertain the truth of such a notion, I have tried the effect of artificial cold, by means of freezing mixtures, on these animals, by which they have been rendered rigid and hard, so that they were doubtlessly frozen; and yet, when thawed, they revived; but, with the exception of one, the death of all of them speedily followed. This result is the more remarkable, as the experiment was made during the height of summer in Malta, and with leeches suddenly taken from water almost tepid, and plunged into a freezing mixture, and when rigid were as suddenly plunged into the tepid water. Now, to return to the argument, if an animal, or even a part of an animal, can be frozen and not deprived of life,—why may not the same happen to a fluid? Why may not its animation, in the same way, be suspended and restored?

The blood, after death, I have often found liquid, and that many hours after death, when cold, but still retaining its power of coagulating. What is to be hence inferred? Does it not follow either that the liquid blood, though coagulable, is dead, and consequently, that coagulability is not a vital property? or that this blood possesses life after the death of the body,—after the muscular fibre has lost its irritability, and the limbs have become rigid, and even partial decomposition has commenced?

It would require little ingenuity to extend and vary these speculations; but, I apprehend, the

farther they are extended, the more difficult it would be to decide and come to a positive and satisfactory conclusion. The question is almost metaphysical. Whichever conclusion is adopted, — whether the blood be a living fluid, — or merely the product of life and pabulum, without vitality, — not much stress need be laid on it, or value attached to it. Our great object should be to investigate its properties by careful observation and accurate experiments, by which method alone our knowledge of it can be extended, and, if possible, our doubts removed. *Anticipation mentis*, as emphatically called by Bacon, is admirably adapted to create doubt, but not to remove it. It is chiefly useful, when it leads to honest and zealous inquiry, by which alone truth, the *interpretatio naturæ* of the same author, can be attained.

15. — *Observations relative to the Question — Is the Appearance of the Blood abstracted, as a Remedial Means, a just Criterion in considering the Propriety of repeating the Operation of Blood-Letting?*

It is of importance, in so momentous a subject as the abstraction of blood, that we should come to some decisive conclusion relative to the above question, — and, whether negative or affirmative, that it should be well founded on facts, and not matter of opinion.

Let us first consider the appearances and qualities of the blood, which are commonly supposed to be indicative of inflammation, and to warrant rather than forbid further blood-letting. They are chiefly the following:—an unusual degree of fluidity of the blood, the instant it is drawn; unusual slowness in coagulating; and, when coagulated, being covered with a buffy coat and cupped.

Relative to these appearances and qualities of blood, experience seems to show, that they are met with in the majority of cases of local inflammation, but with shades, differences, and exceptions, involving much difficulty and perplexity.

1. When the inflammation is violent, rapidly running on to suppuration, and very extensive, as attacking at the same time more than one texture,—or only the same texture, but in different organs,—the blood drawn, I have observed, is neither buffed nor cupped. This I have witnessed most strikingly, in cases of peritoneal inflammation, either pure or complicated, with inflammation of the mucous coat of the intestines, or with diffuse cellular inflammation.

2. In diffuse cellular inflammation, I have often noticed that the blood coagulated rapidly, as rapidly as in health, and yet being unusually liquid, exhibited a slight buffy coat,—provided, as has been already mentioned, the vessel used for holding it, was filled in a few seconds, and instantly put aside and allowed to remain undisturbed.

3. In ordinary cases of inflammation, as of the

pleuræ and lungs, the blood drawn at the commencement of the disease is, occasionally, not buffed nor cupped; but, on repeating the operation the following day, the blood exhibits both these qualities.

4. In cases of inflammation of the mucous coat, whether of the air-passages, or of the alimentary canal, the blood drawn sometimes shows the appearances and qualities enumerated, and sometimes not.

5. Experience does not seem to have established, even generally, any relation in point of degree, between these appearances and qualities, and the intensity of the inflammation. Sometimes the buffy coat on the blood is very thick, and the crassamentum is much contracted, and the symptoms of inflammation are not violent,—and the recovery is not long protracted; and sometimes the reverse of this occurs.

6. In a large proportion of fatal cases, fibrinous concretions, or polypi, as they were formerly called, corresponding to the buffy coat on the blood drawn, are found in the heart and great vessels, and, as well as I can judge from my experience, as often when the lancet has been freely used, or only moderately, or not at all.

The consideration of such facts, I must confess, deprives me of confidence in these appearances, as indications for practice and the repetition of blood-letting, and induces me to conclude, that as a criterion in this way, they cannot be pronounced just or safe.

Let us next consider the appearances and qualities

of the blood, commonly supposed to be connected with a state of the system, as it were, the opposite of the inflammatory, and not to warrant further blood-letting. They are chiefly, as it is supposed, a very soft crassamentum, very little, if at all, contracted,—or the blood remaining liquid,—or the proportion of crassamentum to the serum being unusually small.

So far as I am able to judge, these appearances and qualities of the blood are not proved, by experience, to be connected with the state of the system supposed.

1. In the remittent fever of hot climates, and in cholera morbus, both the common kind and the epidemic, the crassamentum of the blood drawn is generally softer than natural, and little, if at all, contracted; and yet, blood-letting, in these diseases, is not generally injurious,—it is often beneficial,—and occasionally beneficial even when repeated.

2. Blood, without fibrin, in any disease, is very uncommon. I have witnessed it in extreme degree, only in cases of pulmonary ecchymosis or pulmonary apoplexy, as that lesion has recently been called,—and *that* after death, in the cavities of the heart and vessels,—but so soon after death, that it may be taken for granted, that it was not a *post mortem* change,—that it existed previously, and was probably connected with, and may have been partly the cause of the fatal effusion. In such cases, no practitioner, I suppose, would have hesitated, if called in in time, in abstracting blood, especially taking into consider-

ation the plethoric habits of the individuals, to whose cases I allude, and their previous apparent robust health.

In a less degree, I have witnessed the same state of blood in cases of cynanche,—and, in one in particular, after death, which made a strong impression on my mind. This I shall briefly notice, as it appears to me instructive, bearing on the points in question. A young healthy man, previously liable to cynanche tonsillaris, was attacked with sore throat. On admission the same day into hospital, he complained of little excepting some difficulty in opening his mouth and swallowing. An emetic was administered and a purgative. At the evening visit, he appeared to be easier; the pulse 70, soft and rather feeble; no pyrexia. The medical officer left him without the slightest apprehension. At three o'clock, the following morning, he was called to see his patient and found him dead. According to report, he fell early asleep; awoke a little before three in a state of confused alarm and of breathlessness, and expired before aid could be afforded. I was present at the *post mortem* inspection: œdema of the glottis, and epiglottis, and of one of the sacculi laryngis, accompanied with the same state of the uvula, and some effusion in the course of the great vessels, sufficiently accounted for the fatal event. The probability is, that had he been bled, and largely, his life might have been saved; yet, the blood, after death, was almost destitute of fibrin; no fibrin had been poured

out—merely serum; nor did the state of the pulse or of the skin seem to indicate the propriety of blood-letting.

Of the efficacy of blood-letting, in cases of this description, I may mention an instance, analogous to the preceding, excepting in its termination. A patient, with cynanche, in the general hospital at Fort Pitt, suddenly experienced a feeling of suffocation. I was, fortunately, in the ward at the time with an assistant. A vein in each arm was opened as speedily as possible, and a considerable quantity of blood abstracted. As the blood flowed, the dreadful sensation of suffocation subsided; perfect relief was afforded; and he soon recovered completely; but it is worthy of remark that the large blood-letting was followed by violent delirium of short duration.

3. The proportion of the crassamentum to the serum being small, may often be witnessed in acute diseases in their advanced stage, or in acute diseases supervening on chronic of long duration,—or occurring in persons of great delicacy of constitution, of feeble health, or of valetudinary habits. In such cases, no judicious practitioner would think of using the lancet, unless he considered it urgently necessary; and then surely he would not be hindered from using it, even if certain of the blood being below par, in respect to the proportion of the crassamentum it might yield.

Lastly, there is another, and extensive and most important class of diseases (were a classification

formed according to the appearances and qualities of the blood) in which the blood, as far as we have learned from experience, is not apparently altered, as in the continued fever of summer, especially in the south of Europe, differing but little from the ephemera,—as in the early stage of synochus,—as in apoplexy, and tetanus, and many of the diseases strictly belonging to the neuroses, in the treatment of which blood-letting is often useful and sometimes indispensable,—and as in aneurism. These considerations tend to support the former conclusion. It appears to me, that the more experience medical practitioners have, the less confidence they place in the appearances of the blood, in forming their opinion on the nature of a disease and on the mode of treating it. What were the sentiments of the late learned and experienced Dr. Heberden on this matter? He says, “the more we know of the human body, the more reason we find to believe, that the seat of diseases is not to be sought for in the blood, to the sensible qualities of which, they seem to have very little relation; and though it be supposed to hold in all maladies, yet in reality, it is but in very few, that the blood affords the practitioner any useful information.” This is called by Sir Charles Scudamore, in his *Essay on the Blood*, a remarkable passage. In the state of knowledge of the blood, at the time it was written, there can be no doubt, that the practical conclusion with which it ends was perfectly correct; and I fear it is

still so, and believe that it is, though our knowledge of this fluid, since then, has been increased; and that it is so, even more strictly than expressed, even in all diseases, and without the exception of "very few." Is there, I would ask, any one disease, in the considering and management of which, the skilful practitioner considers himself under the necessity of examining the blood? I am not aware that there is one such. In cases of pneumonia and pleurisy, it was, and is perhaps still usual for the practitioner to look at the blood. Suppose the blood is not buffed, and in a few hours the symptoms generally indicate the propriety of abstracting more blood, will the practitioner hesitate to abstract it, merely from the absence of the buffy coat on the blood just drawn? Suppose the blood abstracted in a case of pulmonary inflammation, supervening on tubercular phthisis, is strongly buffed, and the symptoms continue but little if at all mitigated, will the practitioner, on account of the state of the blood, repeat the operation of blood-letting? Most probably he will not. He will consider rather the peculiar state of his patient, and endeavour to subdue the inflammation by other means.

Though I believe the time is not yet arrived, that in relation to practice, we can derive advantage from inspecting the appearances of the blood, yet it is to be hoped that it will arrive; and that it will come, seems probable, if the subject is carefully investigated in a strict scientific manner by those

who are unprejudiced, and are competent to engage in the investigation. Dr. Heberden, in the passage quoted, no doubt, gave the result of *his experience*, which was unfavourable to examining the blood, but as he has expressed it, it does not so appear. His conclusion appears to be founded on a hypothetical basis, the soundness of which may be called in question.

It is an important question for inquiry, whether the blood is, or is not, the seat of some diseases; whether unhealthy blood has not an injurious effect in every instance; and if so, what are its modifications and their effects.

These are legitimate subjects for experimental inquiry. They can be determined only by experiments carefully and laboriously made by the scientific physician, whose taste leads him to engage in such a pursuit, with zeal to support him through it, and opportunities such as a public hospital affords. Such pursuits belong to the higher departments of the profession, and must be conducted in the quiet walks of science. They are, in relation to the ordinary mode of inspecting and examining the blood by practitioners, like the investigations of the philosophical navigator, in comparison with the observations of the trading mariner; the one intent on the natural history and physical laws of the terraqueous globe, the other limiting his attention to the circumstances which obviously, or are supposed to expedite or impede his voyage, and render secure, or

occasion risk to his vessel. As it is advantageous that the two characters should be kept distinct in ordinary navigation, so it seems equally proper, in medicine, that the practical part should not be blended with the scientific, and that the practitioner should keep on his course guided by certain experience, and so far as science gives a steady light, enlightened by its rays, leaving the scientific physician to explain at his leisure, aided by the best methods and instruments of research, the doubtful regions of the profession, and investigate the mysteries of the system in health and disease. Sir Charles Scudamore, in the Essay referred to, is of a different opinion, and strongly recommends the attention of the practitioner to the appearances of the blood, persuaded that they will give "important information of the power and action of the heart and arteries, and of the condition of the system itself." And he points out a number of circumstances, to which, he says, the attention must be directed in blood-letting; as 1st, "the kind of orifice;" 2dly, the stream in which the blood flows; 3dly, the state of the pulse during the bleeding; 4thly, "the apparent density, the colour, and other appearances of the blood on it first being received in the basin;" 5th, "the time in which it coagulates;" 6thly, "the internal appearance of the coagulated blood;" 7thly, "the texture of the coagulum;" 8thly, "the comparative conditions of

the several portions of blood in different cups ;” 9thly, “the appearance and quality of the serum.”

These, I admit, and other circumstances, deserve the attention of the scientific inquirer. I should hope, however, that the mind of the practitioner and his eye would be more intent on the state of the patient, than on that of the blood; and having made the orifice, thought proper in the vein, the blood flowing as he could wish, that he would concentrate his attention on the effect taking place in the living system, if the case be an urgent one, and that he would observe whether there is abatement of pain, or of the sense of oppression; whether the pulse rises or falls as the blood flows: and, reflect, judging from the supposed nature of the disease, and the immediate impression produced, on the extent, to which the abstraction of blood should be carried. For my own part, I must candidly confess, I have never been able to combine in myself the two characters of the practitioner and inquirer at the same time; and that I have always found it beyond my power to watch the pulse and countenance, and feelings of the patient, and study the fleeting appearances of the blood in a satisfactory manner; and consequently, when I have attempted the one, even the investigation of a single property of the blood, I have always been a mere spectator, and have generally requested the practitioner to judge for himself of the quantity of blood which should be

drawn. And, I would recommend the same plan of proceeding to most others; lest, in the ardour and interest of the scientific inquiry, the patient have not that attention paid him, which is his due, and humanity suffer in a cause, which should have humanity for its chief object and end; and the name of science in its turn suffer and fall into disgrace amongst those who do not distinguish between the genuine and false.

III.

AN ACCOUNT OF SOME EXPERIMENTS ON THE BLOOD,
CHIEFLY IN CONNEXION WITH THE THEORY OF
RESPIRATION.

CONNECTED with the theory of respiration and of animal heat, there are many questions of interest respecting the blood, about which physiologists differ in opinion, and which, consequently, are fit subjects for further inquiry.

Some of the more important and fundamental of these questions, I investigated, experimentally, in the winter of 1837–8; the results were communicated to the Royal Society; they were published in the Philosophical Transactions for the latter year; and were, essentially, as follow, exclusive of the ninth and tenth sections, which have since been added.

1.—*Is Blood capable of absorbing Oxygen independent of Putrefaction?*

To endeavour to satisfy myself on this point, on

which, in a former inquiry,* I had arrived at a negative conclusion, in opposition to the commonly received opinion, I have employed two methods of experimenting: one, of agitating blood, recently drawn and rapidly cooled, in common air and in oxygen, in a tube of the capacity of two cubic inches, divided into a hundred parts; the other, of agitating it in larger quantities with the same airs, in the very convenient apparatus employed by Dr. Christison when engaged in a similar inquiry, consisting of a double tubulated bottle of the capacity of thirty-two cubic inches, provided with stop-cocks adapted by grinding, to one of which a moveable bent glass tube was fitted to connect it with a pneumatic trough, and to the other a perpendicular tube surmounted by a funnel.

The blood subjected to experiment in every instance was prepared by the displacement of its fibrin. This was done by agitating it with small pieces of sheet-lead in a bottle filled to overflowing and closed with a cork, enveloped in moist bladder and covered over with the same, tied round the neck of the bottle, so as to exclude atmospheric air, whilst in the act of coagulating and of cooling, and to allow, when cooled, of the withdrawal of the cork.

Prepared thus, and rapidly cooled, I have tried different specimens of blood, venous and arterial, of man, of the sheep, of the ox, and dog; and the results, when the blood has been taken from a

* Edinburgh Medical and Surgical Journal, vol. xxxiv. for 1830.

healthy animal, have been decisive and consistent. In every instance, whether atmospheric air or oxygen was used, after agitation, there was a marked diminution of the volume of air.

In the examples which it may be advisable to bring forward,* I shall confine myself to a few of the experiments I have made on the blood of the sheep.

Using the graduated tube over mercury, sixty-two measures of arterial blood from the carotid artery, agitated with thirty-three measures of common air, produced a diminution of two measures; and sixty-three of the same arterial blood, agitated with thirteen of pure oxygen, a diminution of three; whilst sixty-three of venous blood from the jugular vein of the same animal, agitated with thirty-three of common air, produced a diminution of six; and seventy of this blood, with thirteen of oxygen, a diminution of eight.*

In experiments on a larger scale, using the double-mouthed bottle, in which about ten cubic inches of blood were agitated with about twenty-two cubic inches of air, the results were in accordance with the preceding. Thus, when the arterial blood of the sheep was agitated with common air and with oxygen, on turning the stop-cock of the bent tube there was

* Notwithstanding the frothing attending the agitation of blood in air, the absorption or diminution of volume was ascertained with tolerable accuracy by observing the rise of the mercury in the tube. The experiments were made as nearly as possible under ordinary atmospheric pressure, which was easily effected, as the mercurial pneumatic trough used exceeded in depth the length of the tube.

an absorption in one instance of about $\cdot 3$ cubic inch, and in the other of about $\cdot 4$ cubic inch; and, with venous blood, in the instance of common air, of about $1\cdot$ cubic inch, and in that of oxygen, of about $1\cdot 25$ cubic inch.

These experiments were all made on the blood of the same animal. In experiments on the blood of different individuals of the same species of animal, and on the blood of animals of different species, the results have varied in regard to the degree of absorption, and remarkably so in the instance of the blood of man.

In every instance the absorption or disappearance of a portion of the air has been attended with some change in the colour of the blood; the venous has invariably acquired the florid vermilion hue of arterial blood, and the arterial has had its florid hue heightened.

The air that has been absorbed or which disappears, when atmospheric air has been used, in accordance with the commonly received opinion and the results of Dr. Christison's experiments,* has been found to be oxygen.

Relative to the residual air, when pure oxygen has been used, whether on the smaller or larger scale of experiment, no carbonic acid gas has been detected in it in the most carefully conducted trials. When common air has been employed, then a trace of this acid gas has been found in the residual air

* Edinburgh Medical and Surgical Journal, vol. xxxv. p. 94.

after agitation with the blood, but not exceeding one per cent. at a temperature between 40° and 50° . I shall notice in detail an experiment with each, as the results are of consequence.

Ten cubic inches of venous blood (its fibrin displaced) from the jugular vein of a sheep, rapidly cooled, were agitated in twenty-two cubic inches of oxygen gas, which had been well washed with lime-water. After an absorption equal to about one cubic inch, some of the gas was expelled and passed into lime-water, the transparency of which it did not in the slightest degree impair.

The same quantity of similar blood was similarly treated with common air, which also had been washed with lime-water. After an absorption equal to about one cubic inch, some of the air was passed into lime-water: it occasioned a just perceptible cloudiness; and eighty measures of it agitated with lime-water were reduced hardly to 79.5.

Taking it for granted that this very minute quantity of carbonic acid derived from the blood existed in the state of gas, and not contained in the aqueous vapour, as is possible, it is matter for consideration from whence it was derived,—whether it was formed at the moment by the action of the air on the blood, or, previously existing in the blood, was now merely expelled. Further on I propose to return to this important question.

When I reflect on the results stated above relative to the absorption or disappearance of oxygen, and

compare them with those alluded to formerly obtained, I am not a little surprised at their discrepancy ; and I can only account for it by supposing that it may have been owing in part to the difference of season when the two sets of experiments were conducted. The first were made in Malta, in 1829, in the hot months of July and August, when the thermometer in the open air was generally above 80° and occasionally above 90° . The last have been made in England, and principally in January of the year 1838, when the temperature of atmosphere was occasionally low, the greater part of the time below the freezing point, and often so low as 20° . From what I have witnessed, I am induced to infer, that the higher the atmospheric temperature is, and the less necessity there is for the production of animal heat, the less difference there is between venous and arterial blood, and the less power the former has of combining with oxygen, and of forming or evolving carbonic acid. In Malta I carefully compared the blood of the jugular vein and carotid artery of a sheep during the season mentioned ; when coagulated and still hot, there was no perceptible difference in their colour ; in each it was less florid than the arterial blood of the same animal in England in winter, and less dark than the venous : its hue was as it were a mixture of the two. And in this observation I could not be mistaken ; the circumstances under which it was made precluded mistake ; vessels of the same size were used ; similar quantities of blood were

introduced, and they were seen in the same light side by side. Referring to Dr. Crawford's Treatise on Animal Heat, I find in confirmation, that he had arrived at the same conclusion, more than fifty years ago, from his own experiments on the influence of heat and cold on the colour of the blood, and the quantity of oxygen consumed in respiration. In the most decisive experiments detailed by him, he states, that "the venous blood (of a dog that had been immersed in warm water for half an hour) assumed very nearly the hue of the arterial, and resembled it so much in appearance that it was difficult to distinguish between them."* And, in another experiment, the reverse of this, in which a dog was immersed in cold water at 45° , for about a quarter of an hour, he says, that the blood now taken from the jugular vein appeared to him, as well as to several other gentlemen, who assisted, "to be the darkest venous blood we had ever seen."† In my communication to the Royal Society, in corrobo-

* Experiments and observations on Animal Heat, p. 309. Second edition.

† Perhaps the influence of atmospheric temperature may in some measure help to reconcile the apparently discordant results of numerous observations on the colour of arterial and venous blood; thus on one side Haller says "*Sæpe comparavi arteriæ pulmonalis cavæque venæ sanguinem, cum arterioso sanguine venæ de pulmone reducis, nunquam in multis experimentis, aliquam in colore utriusque sanguinis diversitatem vidi, neque Harveius vidit, neque Guilielmus Cheselden, neque cl. Knight, neque cl. noster auditor Evertsen, neque ipse contrariæ opinionis auctor Hambergerus.*"—*Element. Physiol.* ii. p. 11.

ration of the view taken, I laid too much stress on an inferred higher temperature in winter compared with summer of the deeply-seated parts of animals, specifying that of the heart and rectum of sheep, and which has not been fully borne out by my later observations; those indicating, that the average heat in summer is not less, but is very little higher than in winter. But, as regards the theoretical conclusion, so small a difference of animal temperature, contrasted with the very great difference of atmospheric temperature—the former not exceeding 1° , the latter often $.50^{\circ}$ —can be considered very little less cogent, than if, as I erroneously supposed, the winter temperature of the heart were actually the highest.

2.—*Does the Blood, especially Venous Blood, contain Carbonic Acid capable of being expelled by agitation with another Gas, as Hydrogen or Oxygen?*

In a paper published in the Philosophical Transactions for 1835, Dr. Stevens has answered this question in the affirmative: he maintains that carbonic acid gas exists in venous blood; that it may be expelled by oxygen or by hydrogen, although not by the air-pump; and he supposes that the difference of effect is owing to a peculiar attraction for carbonic acid exercised by these gases.

To endeavour to resolve my doubts on this important point, I have had recourse to the apparatus

already mentioned, viz. the graduated tube with the mercurial pneumatic trough, and the double-mouthed bottle furnished with stop-cocks, &c. as being well adapted for simple and decisive experiments.

By means of the graduated tube I have agitated venous blood in hydrogen over mercury, as about a cubic inch of each, and other proportions, and have left the blood exposed to the influence of the gas for several hours; and I have made similar trials with it, using larger quantities in the double-mouthed bottle, as sixteen cubic inches of each, and also other proportions. The results have been either of the same negative character, or, if different, indicating only the disengagement of carbonic acid gas in an extremely minute quantity. In all the experiments with the graduated tube in which fresh blood was used, whether of man or of the sheep, the fibrin displaced out of the contact of the air, on agitation with hydrogen, there was no sensible increase of the volume of the gas, and no diminution of it when it was transferred to, and shaken with, lime-water. And in the best experiments, on a larger scale, with the double-mouthed bottle, when most attention was paid to all the circumstances likely to insure accuracy, (as in the first instance the exclusion of air from the blood, and in the second the having it of the temperature of the bottle and of the room,) the results have been similar, and negative. I shall describe a small number of experiments, those, the results of which appeared least ambiguous.

Seven cubic inches of sheep's blood from the jugular vein, its fibrin broken up by agitation with lead whilst coagulating, out of contact with air, and cooled under water, were agitated with hydrogen (twenty-five cubic inches), previously well-washed with lime-water, and which, tested by lime-water, after this precaution were found perfectly free from carbonic acid. On turning the stop-cock of the bent tube connected with distilled water, no change of volume was indicated, and the blood was agitated again with the same result. By means of the perpendicular tube distilled water was admitted, and some of the gas expelled; first a cubic inch into a graduated tube filled with lime-water, and next about four cubic inches into a vial filled with distilled water, and in which afterwards a little lime-water was added to the gas; in neither could any traces of carbonic acid be detected; the lime-water remained transparent.

Ten cubic inches of venous blood, taken by a large orifice from the arm of a young man threatened with hæmoptysis, the fibrin broken up in the same manner as the last, and rapidly cooled under water in a running stream to the temperature of the room, 51° , were similarly treated with hydrogen, and with precisely the same result; after having been twice well shaken, on turning the stop-cock, there was no change of volume. The blood was kept in contact with the hydrogen over-night, the stop-cocks closed. The night, that of the 23rd of January, was severely

cold; at 11 o'clock the following morning the temperature of the room was only 45° ; now on turning the stop-cock of the bent tube, the water rose in it to the extent of about one-eighth of a cubic inch.

Twenty-four cubic inches of the mixed arterial and venous blood of the sheep, collected and prepared with similar precautions, were divided into two portions of about twelve cubic inches; one was agitated in the double-mouthed bottle with hydrogen after the introduction of a little milk of lime, the other without this addition. The result in each instance was the same; on opening the stop-cock, after the agitation, the water rose a very little in the bent tube,—about one-twentieth of a cubic inch.

The results of some of the trials already described on the action of oxygen on venous blood, both pure and mixed with azote, in the form of common air, are very consistent with those just detailed on hydrogen. Previously to stating some other results in quest of fresh evidence on the same subject, it may be advisable to notice particularly the power which blood possesses of absorbing carbonic acid.

From experiments which I made on blood and serum in 1824 and 1828*, I inferred that each is capable of absorbing about an equal volume of this acid gas. I now find that when *pure* carbonic acid gas is brought in contact with blood or serum over mercury, and moderately agitated under ordinary

* Philosophical Transactions for 1824. Edinburgh Medical and Surgical Journal, vol. xxx.

atmospheric pressure, the absorption of gas exceeds the volume of the fluid, both in the instance of blood and serum. The results of some experiments are exhibited in the following Table. The majority of them were obtained between a temperature of 40° and 45° ; the three last at about 51° . When the venous and arterial blood, and the serum tried, were from the same animal, the numbers expressing the results are entered in the same line.

Volume of carbonic acid gas absorbed by 100 parts of blood and serum.

No.	Animal.	Venous blood.	Venous serum.	Arterial blood.	Arterial serum.
1	Sheep -	160	—	167	
2	Sheep -	155			
3	Sheep -	—	—	133	
4	Sheep -	142			
5	Sheep -	—	—	150	
6	Sheep -	166			
7	Sheep -	—	—	—	120
8	Ox -	194			
9	Ox -	181			
10	Man -	118			
11	Heifer -	120	—	—	117
12	Sheep -	148	125	141	125
13	Sheep -	—	118	—	118
14	Man -	153			
15	Sheep -	160			
16	Sheep -	—	—	159	

The effect of the absorption of the gas to perfect saturation was on the arterial and venous blood the same; it rendered both very dark; the serum it rendered more liquid, which was well marked by diminished tendency to froth on agitation.

I shall now proceed to notice the trials which I have instituted of agitating blood and serum, to which a known quantity of carbonic acid had been added, with one or more of the gases considered by Dr. Stevens as exerting an attraction on carbonic acid, and by that means expelling it.

From the experiments which I have made on serum, it appears in its healthy state incapable of absorbing oxygen, or of immediately furnishing carbon to form carbonic acid; and in no instance in which I have agitated it with common air or with hydrogen, when obtained from the blood of a healthy animal, has there been any indication of the disengagement of gas; it therefore is peculiarly well fitted for the trial in question.

To nine cubic inches of serum from the mixed blood of the sheep one cubic inch more of serum was added, containing a cubic inch of carbonic acid, with which it had been impregnated over mercury. The mixture of the two was introduced into the double-mouthed bottle without delay, and well agitated with twenty-two cubic inches of common air. On turning the stop-cock there was no change of volume. The serum was transferred, and there was added to it, with as little motion as

possible, another cubic inch of serum, containing the same quantity of carbonic acid. Now poured back into the bottle and agitated, on opening the stop-cock a little air was disengaged; it was collected and found equal to $\frac{4}{100}$ ths cubic inch. The serum was left exposed to the action of the air in the bottle over night, the stop-cocks closed; the following morning on opening the stop-cock of the bent tube no air was expelled; on the contrary, there was a just perceptible rise of water in it. The experiment was carried further: the serum was transferred to a vial and closed, and the double-mouthed bottle was filled with hydrogen. The serum was returned and well agitated with the hydrogen. On turning the stop-cock $\frac{14}{100}$ ths of a cubic inch of air was expelled. It was agitated a second time without further expulsion of air, and left in contact with hydrogen for more than twelve hours without any further effect. Thus it appears that of the two cubic inches of carbonic acid gas introduced into the serum, only one-fifth of a cubic inch was expelled by successive agitation with atmospheric air and with hydrogen.

One cubic inch of venous blood of a man which had absorbed 1.2 cubic inch of carbonic acid, was mixed with twelve cubic inches of similar blood, and agitated with hydrogen in the double-mouthed bottle. A very little air only was expelled, viz. $\frac{4}{100}$ ths cubic inch.

To fifty-five measures (1.1 cubic inch) of venous

blood of a sheep, twenty measures of gas were added over mercury, composed of about equal parts of oxygen and carbonic acid. After agitation about seventeen measures were absorbed, and the blood had acquired a florid hue; ten measures more of oxygen were added; there was no further absorption. The tube was transferred to lime-water and agitated; the residual air was not diminished; it amounted to thirteen or fourteen measures (the froth prevented precision in marking the quantity), which possessed the properties of oxygen, as tested by the taper.

To twenty-eight measures of venous blood of a sheep, which had absorbed twenty-six of carbonic acid, forty-nine of oxygen were added in the graduated tube over mercury: after agitation the blood had acquired the florid arterial hue, and there appeared to be an expansion of one measure. Transferred to lime-water there was an absorption of one or two measures, and no more.

I shall notice one experiment more, in which blood nearly saturated with carbonic acid was exposed to oxygen, a membrane intervening. Forty-seven measures of venous blood, which had absorbed thirty-three of carbonic acid, were introduced into a glass tube half an inch in diameter, closed at one end with gold-beaters' skin, and when filled with blood, at the other end also; it was placed over mercury in a small receiver, and thirty-seven measures of oxygen were added to it, under a diminished pressure of about one

inch. After twenty-four hours there was no change of volume; the blood in the tube had acquired throughout the arterial hue; the gas, thirty-eight measures, transferred to lime-water and agitated, diminished to thirty-two.

The tube was now placed under a receiver, and the air exhausted by the air-pump; a good deal of air was disengaged in the form of froth, and the gold-beaters' skin was so distended that it appeared ready to burst; after three or four minutes air was re-admitted; a notable portion of gas was found free between the membrane and blood; thus showing that in oxygen gas carbonic acid gas is less freely exhaled through a membrane than *in vacuo*.

The results of this second set of experiments are in accordance with those of the first. The inference I am induced to draw from both is rather of a negative kind, and unfavourable to the conclusion of Dr. Stevens, already referred to, at least in a strict and general sense. I think it right to express myself thus reservedly, reflecting on some of the experiments in which a very little carbonic acid gas appeared to be extricated on agitating blood with hydrogen; and believing that Dr. Stevens, and other able inquirers, could not have been misled on a point so little exposed to fallacy.

Relative to the effects which Dr. Stevens refers to the attraction of one gas for another, they appear to me, from what I have witnessed in carrying on the inquiry, to admit of explanation on Dr. Dalton's theory of mixed

gases, and that in no instance is the effect of disengagement of air from a fluid, agitated with another kind of air, greater than were it agitated in a vacuum.

3.—*What is the Condition of the Alkali in the Blood in relation to Carbonic Acid?*

On this point much difference of opinion exists amongst inquirers; some believing that the alkali, or at least a portion of it, is uncombined, or combined merely with water, or with water and albumen; some that it is united to carbonic acid in the state of carbonate; others that it is saturated with this gas, and in the state of bicarbonate.

The subject, it must be confessed, is one of great difficulty, and very perplexing; partly from the nature of the blood, liable to great variations during life, and to rapid change after death; and partly also from the nature of the alkaline carbonates, hardly less disposed to change than the blood itself, from variation of circumstances, and to pass from one degree of combination into another.

The bicarbonate of soda, I believe, like the bicarbonate of ammonia, can only exist in perfection in the solid state. In dissolving, I find, when exposed to the atmosphere, it gives off a part of its acid, and still more when it is agitated with common air, and more still with hydrogen, and in a greater degree the higher the temperature. This is not favourable to the idea that it exists in the blood, especially when

it is considered that this fluid may be exposed to a temperature of 212° without disengaging carbonic acid, of which I have had proof in several trials.

Sulphuretted hydrogen does not expel carbonic acid from the alkalies in solution in water. Bicarbonate of soda dissolved to saturation in distilled water absorbs, I find, 143 per cent. of its volume of this gas ; whilst the serum of blood (it was sheep's that was tried) dissolved 207 per cent., arterial blood 235, and venous 290. This, too, is unfavourable to the same idea ; as is also the large proportion of carbonic acid which blood, it has been shown, is capable of absorbing.

Supertartrate of potash occasions an effervescence, when mixed in substance with a solution of the sesquicarbonate, but not of the carbonate of soda ; and the effect is similar, whether the mixture be made over mercury, air excluded, or in an open vessel exposed to the atmosphere. The supertartrate of potash also, I find, mixed with blood and agitated with common air, acts as with the sesquicarbonate, and occasions a disengagement of air, and both from arterial and venous blood, and from serum ; and the air I have ascertained is carbonic acid.

From these facts, may it not be inferred that the alkali in the blood, in its normal or healthiest condition, is neither in the state of carbonate nor of bicarbonate, but of sesquicarbonate ? The power of the blood to absorb carbonic acid and sulphuretted hydrogen accords best with this view, and some other

important properties of the fluid are, I believe, in harmony with it.

The sesquicarbonate, I may add, seems to be the state of rest of the alkali in combination with carbonic acid, under ordinary circumstances of exposure to the atmosphere. Thus the native compound is the sesquicarbonate, as is also, I believe, the effloresced salt.* And I find that although a solution of the bicarbonate may be brought by the air-pump to the state of sesquicarbonate, it cannot be reduced to that of the carbonate; after it has ceased to give off any air *in vacuo*, it effervesces with the supertartrate of potash; and if evaporated to dryness over sulphuric acid under an exhausted receiver, on being subjected to heat, it disengages carbonic acid gas.

4.—*Does the Blood contain any Gas capable of being extricated by the Air-Pump?*

On this subject also there has been much difference of opinion. Our distinguished countryman Mayow, more than a century ago, stated that blood, especially arterial blood, effervesces *in vacuo*, which he attributed to the disengagement of air, his *Spiritus Nitro-aërius*.† Sir Everard Home, on the

* On exposing carbonate of soda in excess to carbonic acid gas over mercury, the gas is rapidly absorbed with the expulsion of part of the water of crystallization, so as to produce an appearance of deliquescence, and the sesquicarbonate is formed.

† Johannis Mayow, *Opera omnia*. Hagæ Com. 1681, p. 133. The following is the passage referred to—"Si sanguis in vase aliquandiu

authority of Mr. Brande, in 1818, asserted that blood, both venous and arterial, under the exhausted receiver evolves a large quantity of carbonic acid gas, an ounce of blood as much as two cubic inches of gas.* I repeated the experiment shortly after, but without confirming the result; neither by the air-pump, nor by heat applied even to coagulation of blood and serum in close vessels, did I succeed in demonstrating the extrication of this acid.† Since that time the experiment of the air-pump on the blood has been frequently made, and by observers of great accuracy, as by Drs. Duncan and Christison in this country, by MM. Tiedemann, Gmelin, Mitschenlich and Müller on the Continent, and recently by MM. Bischoff and Magnus. With the exception of the last-mentioned inquirers, the results have been negative. MM. Bischoff and Magnus, on the contrary, state that by careful exhaustion they have obtained gas from the blood; the former a small quantity of carbonic acid gas from venous blood, and a very little gas of a different kind from arterial, the nature of which he did not determine;‡ the latter

servatus, in vitrum collocetur, ex quo aer per antliam aeream exauritur, sanguis iste in superficie, quâ idem colorem floridum obtinuit, leniter effervescet, et in bullas assurget. Sin autem sanguis arteriosus adhuc incalescens, in loco aere vacuo positus fuerit, idem mirum in modum expandetur, et in bullulas penè infinitas elevabitur: id quod à particulis ejus exæstantibus, inque motum positis, partim ab aere particulis ejus interspersis oriri verisimile est."

* Philosophical Transactions for 1818, p. 181.

† Ibid. 1823, p. 516.

‡ Commentatio de novis quibusdam experimentis chemico-physi-

a notable quantity, and from arterial as well as venous blood, and not only of carbonic acid, but also of oxygen and azote.*

M. Magnus attributes the failures of former experimenters to their having used pumps of imperfect exhausting power, or to their not having carried the exhaustion sufficiently far. In my early and first trial I employed the air pump belonging to the laboratory of the Royal Institution, which was an excellent one, and which I then believed was in good order; but I might have been mistaken.

To endeavour to satisfy myself on this point, I have had an air-pump constructed for the purpose, under the direction of an able artist, Mr. Ross, of 33, Regent Street, already distinguished for his excellent achromatic microscopes, and it has answered perfectly. When the exhaustion has been carried as far as possible, the difference of level of the mercury in the siphon-gauge has not exceeded a tenth of an inch, and over water has not exceeded the quarter of an inch.

Experimenting with this machine on blood, collected with such precautions as I believe to have been adequate to insure accuracy of results, in a majority of instances the disengagement of gas has

ologicis ad illustrandam doctrinam de respiratione institutis Heidelb. 1837—p. 9 et seq.

* *Annales des Sciences Nat.* tom. viii. p. 79, et seq. contain a translation of M. Magnus' paper, with a figure of the apparatus, &c. used; the original appeared in *Poggendorf's Journal*, vol. xl. part 3.

been rendered manifest, and both from arterial and venous blood.

I shall briefly mention the trials which I consider most conclusive, and which satisfied me in spite of an opposite pre-existing bias.

Vials provided with well-ground glass stoppers were filled with distilled water, deprived as much as possible of air by the air-pump; they were then placed under the receiver and kept in vacuo until all adhering air was removed; the stoppers were now introduced, all air being excluded, and they were instantly immersed in distilled water, which had been well boiled. Thus prepared they were taken to an adjoining slaughter-house, where they were filled with sheep's blood in the following manner, without its coming in contact with the air. For venous blood the jugular vein was exposed; two ligatures were applied to it; the vein was divided between the two, and the upper part, slightly detached, was introduced into a prepared vial under water the instant the stopper was withdrawn, and laid open. The heavier blood proceeding from the vessel of course expelled the water; and when it was supposed to be all expelled, the stopper was restored, and the vial was replaced in the water. In the instance of arterial blood it was collected in the same manner from the carotid artery. In some trials the blood was allowed to coagulate undisturbed; in others the fibrin was detached, and the liquidity of the blood preserved by agitating it, the

instant the stopper was replaced, with some mercury, introduced with the distilled water, and equally deprived of adhering air.

In about half an hour from the abstraction of the blood, in every instance, it was subjected to the air-pump. The instant the stopper was withdrawn the vial was placed in a small receiver on the plate of the pump, and covered with a little larger receiver, and the air as soon as possible exhausted. No appearance of disengagement of gas was perceptible until the exhaustion was nearly complete; then it was sudden, sometimes considerable, even to overflowing, in the form of bubbles, and it continued some time. The results were not distinctly different, that I could perceive, whether venous or arterial blood was used; I am disposed to think, on the whole, that less air was disengaged from the arterial blood than from the venous.

When blood allowed to coagulate in the vessels was tried, the results varied a little, and appeared to me instructive. At first, on exhaustion, only a few particles of air were disengaged; no more, it might be supposed, than were derived from the contact of the end of the stopper. In two experiments such was the appearance for at least five minutes, conveying the idea that no air was extricated; then abruptly a bubble or film burst with some force, as was denoted by the scattering of the particles of blood; and a bubbling commenced and continued, rendering the indications of extrication of gas un-

questionable. And in conformity with this result I may remark, I have never succeeded in obtaining indications of air in the blood in operating on it by the air-pump, if confined in a detached portion of vein, or in the heart of animals, the great vessels, previous to excision, having been tightly secured by ligature. In no instance of this kind have I witnessed any distention, such as occurs if air be admitted previous to the application of the ligatures: clearly indicative, it appears to me, that a very slight compressing force is sufficient to confine the air in the blood; or rather, I should say, prevent its substance assuming the elastic state; and further, the probability, that the quantity of air so condensed is small.

I have stated that in the majority of instances the indications of the disengagement of air from blood *in vacuo* have been manifest. Exceptions, however, have occurred, and those clear and decisive; inducing me to believe that the quantity of air condensed in the blood is variable; and that there are times when it is in excess, and times when in deficiency, and when totally wanting, connected with regularly changing states of the functional system. I hope at a future time to be able to recur to this part of the subject, on which at present I have collected but a few facts. I may add, that such facts tend in part to reconcile some of the discrepancies referred to in the beginning of this section.

5.—*Of the Air or Gases contained in the Blood capable of being extricated by the Air-pump.*

As already mentioned, M. Magnus has stated that these gases are carbonic acid, oxygen and azote in notable quantities. Taking the mean of ten of his experiments on the blood of the horse and calf (five on arterial and five on venous), the total quantity of the mixed gases he obtained was, in the instance of arterial blood, 10·4 per cent. per volume, and in that of venous 7·6 per cent.; the former consisting of about

6·5 carbonic acid,

2·4 oxygen,

1·5 azote;

the latter of

5·5 carbonic acid,

1·1 oxygen,

1·0 azote.

On a subject of so much importance, it is very desirable that the experiments of M. Magnus should be repeated and verified; for until this is done, considering the physiological history of the blood, it will be difficult to avoid doubt, it will be difficult to depend on them with that degree of confidence which is justly due only to well-authenticated facts.

The ingenious apparatus employed by M. Magnus being of complex construction, and not easily used excepting in a well-appointed laboratory, I must leave the repetition of his highly interesting experi-

ments to those inquirers who are more happily situated than myself for engaging in them. I shall limit myself to the detail of some experiments instituted with a view of testing M. Magnus' general results.

As a solution of potassa has the property of absorbing carbonic acid gas, it follows that if mixed with the blood previous to being subjected to the air-pump, it will prove in some measure a test of the kind of air which the blood is capable of affording.

With this intent two vials were prepared, the same as before used, one filled with distilled water the other with a weak aqueous solution of caustic potash, both carefully deprived of air by the air-pump. Observing the same precautions as before, a portion of venous blood from a sheep was received into each of them. When less than half of the water and of the solution, as well as could be guessed, was expelled, the vials were closed with the glass stoppers belonging to them, and instantly immersed in water, and as soon as possible subjected to the air-pump. The results of exhaustion in the two instances was perfectly distinct. From the blood mixed with water gas was disengaged; there was a continued ascension of bubbles. From the blood mixed with the alkaline solution no gas was liberated, excepting a bubble or two, which might fairly be considered as entangled air derived from contact of the blood with the stopper.

A similar comparative trial was instituted with

arterial blood of the sheep and the results also were similar and equally well marked.

Considered as test experiments, the first inference from these results is, that carbonic acid gas, or an air absorbable by a solution of potash, is disengaged both by venous and arterial blood in vacuo; and next, that no other gas is disengaged from either of them, neither oxygen nor azote, each of which is unabsorbable by the solution in question. I have repeated the experiment twice on the venous blood of man, and twice on the venous blood of the sheep, and twice on the arterial blood of the latter, without variation of results; and they are more to be depended on, as the alkali has the effect of preserving the blood liquid.

I may mention another method which I have employed as a test experiment; and, first in relation to carbonic acid. If the blood contain a notable portion of this gas capable of being extricated by the air-pump, it necessarily follows, that when subjected to the action of the air-pump, and deprived as far as possible of its fixed air by this means, it will be capable of absorbing a larger quantity of carbonic acid than previous to the exhaustion. The following Table contains the results of three comparative trials on the venous blood of the sheep, its fibrin separated or detached in the usual manner.

Table showing the Absorption of Carbonic Acid Gas by

Venous blood subjected to the air-pump.		
Volume of blood used.	Volume of carbonic acid introduced.	Volume of gas absorbed.
27	46	38
27	45	39
27	46	35
Venous blood not subjected to the air-pump.		
Volume of blood used.	Volume of gas introduced.	Volume of gas absorbed.
27	47	37
27	45	39
27	45	34

Although I have thought it right to notice these results, and although they are in accordance with the preceding, I do not attach value to them, excepting as tending to show that the quantity of carbonic acid gas extracted by the air-pump, when the blood affords it in vacuo, is small.

The same mode of reasoning suggested comparative trials of the absorbent power of arterial blood for oxygen, before and after exhaustion by the air-pump, as a further test experiment, whether arterial blood contain oxygen in a free state, that is, admit-

ting of being extricated by the removal of atmospheric pressure. The result of this trial also has been negative; its power of absorbing oxygen has not appeared to be at all increased by exhaustion; this at least was the result of one experiment carefully conducted.

6.—*Is any Oxygen contained in the Blood not capable of being extricated by the Air-pump?*

Before I was acquainted with the researches of M. Magnus I had instituted some experiments to endeavour to determine whether any oxygen in a free state, or in a condition approximating to that state, exists in the blood, and especially in the arterial, admitting of being detected by means of substances possessing a strong attraction for oxygen, or of being expelled by substances of greater solubility in blood than oxygen. Hydrogen, phosphuretted hydrogen, sulphuretted hydrogen, nitrous oxide and nitrous gas, it appeared advisable to try, as belonging to the first class of substances, and carbonic acid gas as belonging to the latter.

The results with hydrogen, sulphuretted hydrogen, nitrous oxide and carbonic acid gas, were of a negative kind. Neither using arterial blood, nor blood which had been agitated with oxygen, and which had absorbed or made to disappear a certain quantity of this gas, could I detect any indications of its presence either by combination or expulsion. In the in-

stances of nitrous oxide and carbonic acid, however, it may be worthy of remark, that the blood which had been agitated with oxygen absorbed less of either of these gases than it did before it was so treated.*

The results with phosphuretted hydrogen, the spontaneously inflammable species, were of an ambiguous kind, not sufficiently clear to deduce from them any satisfactory conclusion. In one trial, serum of the venous blood of the sheep absorbed nine per cent. of this gas; venous blood 11·3 per cent.; and arterial 5·8 per cent.†

The results with nitrous gas were of a different kind, and may be deserving of being specially noticed.

The blood used was that of the sheep prepared in the usual manner. The experiments were made during the very cold weather which prevailed in the beginning of the year already mentioned, and the difference of colour between the venous and arterial blood was very strongly marked.

* Nitrous oxide I find is absorbed in about the same proportion by venous and arterial blood, and by the serum of blood, and also in about the same proportion by water. Thus, at the temperature of 45° over mercury, using the blood and serum of the same animal (the sheep), thirty-two measures of each absorbed twenty-two measures of this gas, and thirty-two of distilled water absorbed 21·5.

† Supposing the gas decomposed—the phosphorus uniting with the oxygen in the blood, the apparent smaller absorption by arterial than by venous blood is what might be expected, on the idea that the former kind of blood contains most oxygen.

(1).—*On arterial blood.*

1. Fifty-three measures of this blood were agitated over mercury with forty-six measures of nitrous gas; there was a diminution of volume of seventeen measures.

2. Fifty-three measures of the same blood (another portion) were agitated with nine of oxygen; two measures were absorbed.

3. Fifty measures of this blood, so treated with oxygen, were agitated with forty-seven of nitrous gas; there was a diminution of twenty-two measures.

(2).—*On venous blood.*

1. Fifty-three measures of this blood were agitated with fifty of nitrous gas; there was a diminution of ten measures.

2. Fifty-three measures (another portion) were agitated with ten of oxygen; five measures were absorbed.

3. Fifty-one measures of blood so treated were agitated with forty-nine of nitrous gas; there was a diminution of seventeen measures.

The residual air in each instance was examined and was found to be a mixture of nitrous gas and azote with carbonic acid gas. The azote, it may be presumed, was introduced with the nitrous gas; it was in the same proportion as that which adulterated it, viz., about four per cent. That the residual air was free from carbonic acid was inferred from the

circumstance, that in comparative experiments with and without addition of a portion of solution of caustic alkali, there was no difference in the proportion of nitrous gas absorbed: and it was corroborated by another circumstance, viz., that after the absorption of the nitrous gas, the blood was capable of absorbing seventy per cent. of carbonic acid gas;* and farther by the result that when nitrous gas is added not to saturation, the whole of it is absorbed.†

As regards the blood itself, the colour of both venous and arterial was altered; both were rendered darker and browner, as if a minute quantity of nitric acid had been added to them, a change long known to be occasioned by nitrous gas. In the degree of change there was however a difference; in the instance of arterial and of oxygenated arterial blood, it was more strongly marked than in that of the venous.

In conjunction with the unsuccessful attempts with the other gases already mentioned, do not the results

* To some venous blood of a sheep which absorbed 182 per cent. of carbonic acid gas, so much of a solution of pure hydrate of potash was added, that it absorbed 218 per cent. of the acid gas; seventeen measures of this blood with excess of alkali, agitated with fifty-one of nitrous gas, absorbed 5.5 measures; sixteen of the blood without the excess of alkali absorbed five measures: thirty-four measures of carbonic acid gas were added to the latter, the excess of nitrous gas being left in the tube; on agitation twelve measures of the carbonic acid were absorbed.

† In one experiment eighty-four measures of the venous blood of the sheep were agitated with ten of nitrous gas over mercury; the whole of the gas was absorbed: twelve of oxygen were added; on agitation again there was no further absorption.

just described indicate that a portion of oxygen exists in the blood, not capable of being extracted by the air-pump, and yet capable of entering into combination with nitrous gas, and which exists in largest proportion in arterial blood? Unless this conclusion is adopted, it must be supposed either that the nitrous gas which disappears is decomposed, or that it combines directly with the red particles; neither of which suppositions is well supported by facts. The circumstance that no azote is disengaged, is not favourable to the idea that the nitrous gas is decomposed; and the difference of effect in the instances of venous and arterial blood, and, after and before agitation with oxygen, is not in accordance with the notion of direct combination. I may mention another fact which seems to have the same bearing: serum, which does not absorb oxygen, I find also does not absorb nitrous gas, excepting in about the proportion in which water absorbs it: and, further, in corroboration, I may mention, that as blood putrefies, whether arterial or venous, its power of absorbing nitrous gas diminishes; and that it is also diminished by being agitated with phosphuretted hydrogen. Thus the arterial blood of a sheep, which before agitation with phosphuretted hydrogen absorbed 45·3 per cent. of nitrous gas, after agitation with it absorbed 7·4 per cent. less; and, after it had become putrid, it absorbed twenty per cent. less. According to my observations, arterial blood does not lose its peculiar florid hue under the action of

the air-pump. Is not this also in favour of the above inference, that a portion of oxygen is retained by the blood resisting extraction by the air-pump? I find also that when venous blood is agitated with oxygen and subjected to the air-pump, it, in like manner, retains its acquired florid vermilion hue, and likewise a power of absorbing an additional quantity of nitrous gas.

7.—*When Oxygen is absorbed by the Blood is there any production of Heat?*

To endeavour to determine this point, of so much interest in connexion with the theory of animal heat, a very thin vial, of the capacity of eight liquid ounces, was selected and carefully enveloped in bad conducting substances, viz. several folds of flannel, of fine oiled paper, and of oiled cloth. Thus prepared, and a perforated cork being provided, holding a delicate thermometer, two cubic inches of mercury were introduced, and immediately after it was filled with venous blood, kept liquid as before described. The vial was now corked and shaken; the thermometer included was stationary at 45° . After five minutes that it was so stationary, the thermometer was withdrawn, the vial closed by another cork was transferred inverted to a mercurial bath, and $1\frac{1}{2}$ cubic inch of oxygen was introduced. The common cork was returned, and the vial was well agitated for about a minute; the thermometer was now introduced, it rose immediately to 46° , and con-

tinuing the agitation it rose further to 46·5, very nearly to 47°. This experiment was made on the 12th of February, 1838, on the blood of the sheep.

On the following day a similar experiment was made on the venous blood of man. The vial was filled with eleven cubic inches of this blood, its fibrin broken up in the usual manner, and with three cubic inches of mercury; the temperature of the blood and mercury was 42·5, and the temperature was the same after the introduction of three cubic inches of oxygen. The temperature of the room being 47°, a fire having shortly before been lit, the vial was taken to an adjoining passage where the temperature of the air was 39°. Here the vial was well agitated, held in the hand with thick gloves on as an additional protection. After about three quarters of a minute the thermometer in the vial had risen a degree, viz. to 43·5.

On the 14th of the same month a third experiment was made on venous blood from the jugular vein of a sheep. The vial was filled with 3·5 cubic inches of mercury and eleven cubic inches of blood. The thermometer in the bottle, left five minutes, was stationary at 49°; the temperature of the mercurial bath was 49°; the air of the room was 52°; a thermometer with its bulb moistened was 45°, which I mention because the outer covering of the vial was moistened with some blood which had overflowed. After three cubic inches and a half of oxygen had been introduced, before agitation, the

thermometer was still 49° . The bottle was briskly shaken for about half a minute; now, on observing the thermometer, it was found at 50° ; the vial was again agitated; there was no further increase of temperature: after ten minutes it had fallen to 49° .

I shall relate one experiment more, and that on arterial blood. It was made on the 14th of February, and in the same manner as those on the venous blood. Before and after the introduction of the oxygen, the blood, which was from the carotid artery of the sheep, was 45° ; after agitation with oxygen it rose to 45.5 : this was done when the temperature of the air was 39° .*

In a former part of this paper I proposed to recur to the question, Is the fixation of oxygen in the blood attended with the formation of carbonic acid gas? The change of colour accompanying the fixation of oxygen by the blood, so different from that produced by carbonic acid, and the effect of

* Sir Charles Scudamore, in his *Essay on the Blood*, at page 59, states that venous blood cools much more slowly in oxygen gas than in atmospheric air; that the same blood divided into two cupping glasses "after an interval of eight minutes from the beginning of the experiment," exhibited a difference of eight degrees,—that exposed to oxygen being 85° , that to atmospherical air 77° . As in this instance, the surface of the blood only was exposed and there was no agitation,—the effect described could not have been from the absorption of oxygen;—an appreciable portion of oxygen could not have been absorbed in so short a time. How it was accomplished (the retarded cooling) I shall not attempt to explain; I apprehend there must have been some mistake, or fallacy, either in observing or recording the result.

nitrous gas before and after, seem to be most in favour of the idea, that the oxygen, in the first instance, is simply absorbed, and that the heat evolved is merely the effect of its condensation; or, that if any of it enters into immediate union with the carbon, it is only a small part of the whole.

8.—*Theoretical Remarks.*

Should the results detailed in the preceding pages be confirmed on repetition, they can hardly fail to have some effect on the theory of respiration and animal heat.

As regards the former, they appear to me to tend to show that the lungs are absorbing and secreting, and perhaps exhaling organs, and that their peculiar function is to introduce oxygen into the blood and separate carbonic acid from the blood.

As regards animal heat, they appear to favour the idea, that it is owing, first, to the fixation or condensation of oxygen in the blood in the lungs in conversion from venous to arterial; and secondly, to the combinations into which it enters in the circulation in connexion with the different secretions and changes essential to animal life.

In illustration of what I imagine the secreting power of the lungs, I may mention the difference of effects in an instance of death by strangulation, and another by exhaustion of air from the lungs by the air-pump. A full grown guinea pig was the sub-

ject of experiment in each. The one killed by strangulation died in about a minute after a cord had been drawn tightly round its neck; the other, placed on the plate of the air-pump and confined by a receiver just large enough to hold it, lived about five minutes after the exhaustion had been commenced, the pump the whole time having been worked rapidly. The bodies were immediately examined. The heart of the strangled animal was motionless; it was distended with dark blood; twelve measures of the blood, broken up and agitated with twenty-nine of carbonic acid gas, absorbed eighteen measures, or 150 per cent. The heart of the other guinea pig was also distended with blood, but of a less dark hue. Its auricles were feebly acting; the lungs were paler than in the former and more collapsed: ten measures of blood from the heart, broken up and agitated with fifty of carbonic acid gas, absorbed thirty-seven measures, or 370 per cent.!

Further, in illustration of this supposed secreting power of the lungs, I might adduce the condition of the blood in disease, and in instances in which I have examined it after death from disease, in the majority of which I have found the blood loaded with carbonic acid, as indicated both by the disengagement of this gas, when the blood was agitated with another gas, and by the comparatively small proportion of carbonic acid which the blood was capable of absorbing. This condition of the blood,

in relation to carbonic acid, I believe to be one of great interest and importance, and capable, when further investigated, of throwing light on many obscure parts of pathology, and especially on the immediate cause of death, and that happy absence of pain in dying which is commonly witnessed.

As regards an exhaling power, which I suppose the lungs may possess, I conceive it may be exercised occasionally under peculiar circumstances—circumstances, in the first instance, favouring an accumulation of carbonic acid gas in the blood, as undue pressure of any kind, and, in the second instance, circumstances of a different nature, connected with the removal of undue pressure, admitting thereby the excess to pass off.

The view which I have alluded to relative to the production of animal heat, is, I believe, capable of explaining very many particulars of animal temperature in different classes of animals, and both during life, in health, and disease, and in a state of hybernation, and after death. If correct, this it must necessarily do, theory being merely an expression of facts, and truths in nature being perfectly consistent.

9.—*On the Difference of Colour of Venous and Arterial Blood.*

Of that difference of colour between venous and arterial blood, which, it appears, was first specially

demonstrated by Lower,* at least fifty† years after the great discovery of the circulation of the blood by

* Mayow, the worthy contemporary of Lower, thus notices Lower's observation of the difference of colour of the two kinds of blood, in connexion with his peculiar views of its cause :—

“Atque prædicta, magis adhuc ex eo confirmantur, quod sanguis, qui sub-atro colore pulmones intravit, idem magis floridus, rutilusque qualis est sanguis arteriosus, ex iisdem redit, prout à *Clariss. Lowero* in vivi-sectionibus observatum est; idemque ostendit mutationem istam in cruoris massa factam, non tam à comminutione ejusdem in pulmonibus, quam ab aere ei admixto provenire: etenim sanguinis venosi in vase excepti, superficies summa, quæ aeri exposita est colorem coccineum, floridumque acquirit; cum tamen sanguis iste in fundo vasis sub colore atro-purpureo apparet, qui tamen idem aeri expositus, post breve temporis spatium colorem rutilum induet. Ut mirandum non sit, si sanguis in pulmonibus, ubi viz. aer per particulas ejus quasque diffusus, cum eodem intimè permiscetur, per totum floridus reddatur.” De sal-nitro et spiritu nitro-aëreo. cap. viii. De spiritu nitro-aëreo quatenus ab animalibus hauritur, in Op. om. p. 131.

† The difference of colour of the two kinds of blood had been long before noticed; in Harvey's time it was a subject of controversy; he admitted the truth of the then, as now, common remark, that the blood from an artery of a living animal is more florid than from a vein, which he attributed to accidental circumstances connected with the two, maintaining at the same time, that the colour of the two kinds of blood was essentially the same, “Si sanguis è rescissâ arteriâ liberè profluens acetabulo accipiatur, venosus apparebit.” He adds, “multo floridior sanguis in pulmonibus, et exinde exprimitur, quam in arteriis reperitur.”

The merit of Lower consists in having associated the colour with its cause, and that not hypothetically; but experimentally, as described by Mayow, and as detailed by himself in his excellent little treatise, *De Corde et de Motu et Colore Sanguinis*, published in 1669. The following is his general conclusion on the point in question—“Quare sanguinem in suo per pulmones transitu aërem haurire, ejusque admixtioni floridum suum colorem omnino debere maximè verisimile est; postquam autem in habitu corporis et viscerum parenchymatis aër rursus à sanguine magnâ ex parte avolavit, atque

Harvey,* two principal explanations have been offered by Physiologists. By the majority, following Mayow, the bright vermilion hue of arterial blood has been attributed to the influence of the oxygen of the atmospheric air inspired on the darker venous blood, and the formation and separation, either directly or indirectly, of carbonic acid. By Dr. Stevens, recently, a totally different view has been taken of the subject. He considers the salts of the serum more concerned in producing the florid hue of arterial blood, than oxygen, believing that they impart this colour to the colouring matter, hematosine; that venous blood acquires its dark colour in the extreme systemic circulation; and that it loses the dark colour and resumes the florid hue, in its extreme pulmonary course; in the one instance acquiring carbonic acid, in the other losing carbonic acid, in the latter expelled by the attraction of the oxygen inspired.†

per poros corporis transpiravit, sanguinem venosum illo privatum obscuriorem et nigriorem illicè apparere, rationi pariter consentaneum est."—Op. cit. p. 170.

* In 1615, it appears, Harvey first publicly taught the doctrine of the circulation of the blood; he was then æt. 37; the discovery was probably made about the time of his return from Italy, in 1602, as he told Boyle, that he derived the first idea of it from considering the use of the valves, to which his attention was probably specially directed whilst studying at Pavia, by Fabricius ab Aquapendente, who claimed for himself the discovery of them in the veins, and sagaciously conjectured their use in the extremities.

† The following passage, expressing Dr. Stevens's peculiar views, is from his paper, entitled "Observations on the Theory of Respiration," in Phil. Trans. for 1835.

"I have said, that black is the natural colour of the colouring

The results of the experiments which I have already given, appear to me no wise reconcilable with this theory of Dr. Stevens ; but perfectly in accordance with the older views which he controverts. Of this description is the fact, that venous blood does not acquire the arterial hue when acted on by the air-pump, even when carbonic acid gas is disengaged ; and that venous blood does acquire it, from the absorption of oxygen, without the disengagement of carbonic acid ; and acquires it even when agitated with a mixture of carbonic acid gas and oxygen gas, absorbing a small portion of the latter, and a large portion of the former.

I apprehend, that the circumstances which Dr. Stevens has brought forward in support of his argument admit of explanation consistent with the commonly received opinion of the part performed by oxygen in respiration. I shall mention a few, the more important of them, with others tending, as I believe, to elucidate the subject.

matter ; but when this agent is diffused in a saline fluid, such as the serum, it is of a bright scarlet tint, which is in fact the natural colour of arterial blood. When carbonic acid is added to this blood in the extreme circulation, it becomes dark-red ; but when this acid is removed in the pulmonary organs, the blood then resumes its natural scarlet or arterial colour ; and this, as I have said, is produced not directly by oxygen, but chiefly, if not entirely by the action of the salts of the blood on the colouring matter. Oxygen, it is true, changes the colour from venous to arterial : this, however, is effected, not by any specific action, but by the removal of the carbonic acid, which had been the cause of the dark colour in the venous circulation."

Certain agents, as it is well known, darken the blood, whether venous or arterial, such as most of the acids, the fixed alkalies, and ammonia in aqueous solution, and water itself. In every instance in which I have examined blood so darkened, I have found more or less of the colouring matter dissolved, and the form of the blood-particles, as ascertained by the microscope, more or less altered; commonly, greatly reduced in size, and from discs commonly converted into globules.

Certain other agents have a contrary effect, and, on immediate admixture, apparently brighten the blood. Amongst the insoluble, or slightly soluble substances, may be mentioned, magnesia and its carbonate, white oxide of arsenic, boracic acid, viz. the hydrate in its crystalline form. Amongst the soluble substances may be specified those well-known ones, the neutral salts, common salt, the carbonated alkalies, sugar.

The first class do not appear to produce any change in the blood-corpuscles; I have not been able to detect any. Probably the effect is owing partly to the introduction of air mechanically entangled; partly to the separating of the blood-particles also mechanically; and partly to the white particles of the substances reflecting the colour of the blood-particles interspersed.

The second class,—(for instance, mixed with the blood in concentrated aqueous solution,) do not appear, more than the preceding, to alter immedi-

ately the form of the blood corpuscles (this is the result of all my trials); but to separate them from each other and prevent their coalescing.* And, may it not be to this circumstance, that in relation to colour, they owe their apparent brightening agency?

We know, that the more concentrated colouring matter is, the darker is its hue; that the more diffused the coloured particles are, and the more mixed with white particles; or the purer the white ground is, on which they are spread, so much lighter and brighter is the effect. The great masters of colouring of the 16th century practised their art, it is understood, on these simple principles; using always transparent colours, and never black, and so imitating the rich mellow, and as it were living colours of natural bodies, amongst which black is almost entirely confined to coal and charcoal.†

I shall give in detail a few observations in support of the preceding statement. In each trial which I shall mention, the cruor of blood was used, that is, blood deprived of its fibrin, prepared in the manner already described.

* It is well known, that when a drop of cruor (serum with blood-particles suspended in it) taken from crassamentum, recently formed, is placed under the microscope, the particles collect together in groups and coalesce by their broad surfaces, forming little piles, or rouleaux—and this both in the instance of venous and arterial blood—which coalescence (the one alluded to above) is prevented by a saline solution.

† These remarks are applicable to hematosine, which is apparently black only in mass; in powder, or seen in small pieces by transmitted light, it is red.

Experiment 1.

Equal parts of the cruor of the venous blood of an ox, and water, were mixed together. The colour of the mixture was darker than that of the cruor alone, especially by reflected light. Under the microscope, no blood discs could be discovered in the mixture,—only small globules, about one-third of the diameter of the discs, of faint appearance, as if transparent. Solution of salt added to the mixture rendered it of a brighter red—light scarlet. Now, under the microscope, the particles appeared smaller, and well defined, and distinct, so that they were most easily seen. The distinctness of their appearance was not increased by the addition of corrosive sublimate.*

In similar trials with solution of ammonia, of

* When a portion of crassamentum is put into water, its hue is presently darkened; and the same effect is produced by allowing water to run over the crassamentum. Dr. Stevens is of opinion that this is owing to the removal of the saline matter by the water. According to my observations, in conformity with those stated in the text, it is occasioned by a change in the blood-corpuscles themselves, and a partial solution of the colouring matter. When the darkened crassamentum is taken from the water and put into a solution of salt, it immediately acquires a brighter hue. This Dr. Stevens refers to the restoration of the saline matter. I apprehend it is owing to the effect of the saline matter chiefly on the altered blood-corpuscles,—by which they reflect more light.

It is surprising how slowly the colouring matter is removed from the crassamentum by water, when the mass is not broken up; the water appears to penetrate very slowly through it; and the colouring matter, even when dissolved, seems to be retained in the mass as in a sponge.

potassa, of hydrocyanic acid and strong acetic acid, the effects on the blood seemed to be very similar; the appearances were much the same, whether viewed with the naked eye or with the microscope; the colouring matter was dissolved, the forms of the discs were changed, and a darkening effect was produced.*

Experiment 2.

Equal parts of a strong solution of refined sugar and cruor were mixed together. The mixture immediately appeared of a brighter hue; at and near the upper surface scarlet; towards the bottom light brick-red. After twenty-four hours, it appeared generally of a scarlet hue, until it was agitated, when the interior and central portion of the mixture was found to be dark. Under the microscope the blood-discs were perceived to be unaltered. The effect of admixture of common salt, of nitre, of muriate of ammonia, of carbonate of potash, was very similar; as viewed with the naked eye and with the microscope. After twenty-four hours, in the instance of common salt and nitre, the solution was hardly tinged by the colouring matter of the blood, and the blood-corpuscles remained distinct in form,—near the surface, of a vermilion hue,—within of a darker hue.

* In all these instances, the solution of the colouring matter, viewed by transmitted light, was rich red, approaching the ruby hue; in particular instances, compared one with another, there were slight differences of colour.

In the instance of muriate of ammonia and carbonate of potash, a little of the colouring matter was dissolved, and the colour of the mixture was less bright, and the blood discs were a little smaller.*

Experiment 3.

A little muriatic acid and cruor were mixed together, the former not in sufficient quantity to coagulate the serum. The mixture presently acquired a darker hue. Under the microscope, the blood-corpuscles appeared somewhat altered in form,—they were less distinctly disc-like,—they approached more to the globular, and had minute particles (probably of air) adhering to them. On the addition of more and of stronger acid,—effecting the coagulation of the serum,—the blood-corpuscles were found diminished in size and irregularly globular; they were examined in a state of dilution by the addition of a little saturated brine.

Do not these results warrant the preceding inference? All the facts with which I am acquainted appear to me, to indicate, that the colour of the blood, whether venous or arterial, that is, dark or florid, is independent of the saline matter in the serum, considered in relation to agency; and that

* I have not found an aqueous solution of hematosine brightened in colour by common salt. It contains no globular particles suspended in it, like the mixture of cruor and brine, to enable the effect to be produced,—unless the common salt acted chemically, which it does not appear to do.

according to the commonly received view, oxygen is the cause of the bright hue of the arterial fluid,—and its consumption and conversion into carbonic acid, the cause of the dark hue of the venous,—the saline serum being negative in regard to colour, its chief use being to preserve the red globules from injury, prevent the solution of their colouring matter, retain their forms unchanged, and to bear them in their course through the circulation.*

Why oxygen should render the colouring matter of the blood scarlet; why its conversion into carbonic acid should change the scarlet to a darker hue; and what is the true shade of colour, of the colouring matter of the blood, are questions, to which, I believe, the present state of our knowledge does not admit of a reply in a satisfactory manner. The colouring principle of the blood-corpuscles, in its perfect state, can hardly be considered precisely the same as the hematosine extracted from them; the former is not soluble in serum,—the latter is; nor have I found the colour of the latter in solution affected by oxygen; consequently, we cannot, with

* The conclusion relative to the colour of the blood,—and especially its modification by neutral salts, acids, and alkalies, and other agents, to which my experiments have led me, is very similar to that of Dr. Wells, deduced by him from a somewhat different train of experiments and reasoning, more than forty years ago, as described in his very ingenious paper, entitled “Observations and Experiments on the Colour of the Blood,” published in the *Philosophical Transactions* for 1797. This paper, which has been much overlooked of late years, is every way worthy of the very original mind of its author, and will, even now, well repay the perusal.

advantage, predicate the properties of the one from the other.

10.—*Do the Arteries after Death contain Air?*

Harvey, from the results of his researches came to the conclusion, that the arteries after death, as well as during life, are destitute of air.* This too, was the conclusion of Prochaska; he opened the arteries under water, and could find no air in them.† Amongst very recent inquirers, Professor Burdach is, I believe, peculiar for maintaining an opposite opinion. He says, “*Les artères des cadavres sont*

* The passages from Harvey bearing on this, are in many respects interesting and instructive. Discussing the question which was agitated amongst the ancients, whether the arteries are the channels of air or of blood, he remarks, “*Sanguinem in arteriis contineri, et arterias solum sanguinem deferre tum experimento Galeni, tum in arteriotomia, tum in vulneribus manifestum est: cum ab una arteria dissecta, hoc etiam Galenus affirmat, plurimis in locis, unius semihoræ spatio totam massam sanguinis ab universo corpore, magna et impetuousa profusione exhaustam fore. Experimentum Galeni tale est: Si (inquit) funiculo arteriam utrimque ligaveris, et medio rescisso secundum longitudinem, quod inter duas ligaturas in arteria comprehensum erit, nihil præter sanguinem reperies: et sic probat sanguinem solum continere. Unde etiam similiter nobis ratiocinari licet: si eundem sanguinem, qui venis similiter ligatis, et rescissis inest, inveneris in arteriis (quem admodum in mortuis, et aliis animalibus sæpius ego expertus sum) eâdem ratione similiter concludere nos possumus, arterias eundem sanguinem, quem venæ, et nihil præter eundem sanguinem continere.*” *Exerc. Anat. Proem.*

† *Disquisitio Anatomico-physiologica organismi corporis humani ejusque processus vitalis, p. 87.*

vides de liquide, et par conséquent pleines d'air, qui doit s'être dégagé du sang."* In support of this inference, he refers to the observations of Redi and Caldesi, of Haller, Spallanzani, and of Blumenbach, according to which bubbles of air are not uncommon in the blood of reptiles, even when circulating in their vessels; he adduces the fact of the readiness with which air is absorbed by the blood; and, the additional fact, as he believes, of blood readily giving off air under the exhausted receiver. These are the circumstances on which he founds his opinion, and comes to the conclusion, "Si Prochaska n'a point vu de bulles s'en élever lorsqu'il les ouvrait sous l'eau, il faut que quelque erreur d'observation se soit glissée dans cette expérience."

Theoretically viewed, especially taking into account the great difficulty with which air is separated (when it is possible) from the blood, by the air-pump, and never, as far as I have observed, in its vessels,—this conclusion of Professor Burdach is open to many doubts; doubts which are not removed by his after reflections.

The question is necessarily one of difficulty, as well as of much physiological interest in relation to its bearings; and, of a nature, as it appears to me, admitting of solution only by accurate and strict observation independent of speculative views. If it is a fact, that the arteries contain air after death, and

* *Traité de Physiologie considérée comme Science d'Observation.* Traduit par A. Jourdan. Tom. vi. p. 285.

that that air is carbonic acid, it will be easy to account for it by the results adverted to in the preceding 8th section. To endeavour to determine this point, as regards matter of fact, I have availed myself of the opportunities which have offered in the General Hospital at Fort Pitt, to institute on the cadaver the examination necessary. The inquiry was begun in the month of September, 1838; and in every fatal case that afterwards occurred, with the exception of four, during a period of six months' search was made for air. As most convenient for examination and best fitted to give accurate results, the internal jugular vein (thinking it advantageous to extend the inquiry to this vessel) and the carotid artery in the middle of the neck, were selected for the purpose. In every instance the vessels were exposed with caution,—as much care as possible being taken to avoid the wounding of branches. To determine if any air was in the vein, not satisfied merely with the appearance of the vein, a puncture was always made into it, and some blood pressed out: and, to determine the same of the artery, a portion of it, about an inch in length, where it gives off no branches, was included between two ligatures; cut out so secured; and transferred to water and there opened. The description of cases in which the trials were made, is shewn in the two following Tables, in which also are noticed some other particulars, which for the sake of accuracy it appears desirable to specify.

TABLE I.
Cases in which the Carotid Artery and Jugular Vein were examined for air, before the removal of the Calvaria.

No.	Age.	Fatal disease as determined by autopsy.	Time of death.	Time of examination (in hours) after death.	REMARKS.
1	26	Pulmonary hemorrhage .	Sept. 17	12	Vein distended with cruor.
2	45	Inflammation & suppuration of joints	" 19	1	Vein distended with liquid blood, which quickly coagulated on exposure.
3	23	Phthisis pul.—pneumathorax	" 24	18	Vein distended with cruor, and contained two or three minute bubbles of air.
4	37	Cerebral hemorrhage	" 25	12	Vein distended with cruor.
5	31	Ascites; indurated spleen & liver	Oct. 7	4½	Vein distended with fluid blood; when opened, it flowed in a jet, it may be inferred, from abdominal pressure; it afterwards coagulated.
6	32	Peritonitis .	" 7	17	Very little blood in vein.
7	31	Phthisis pul. .	" 10	19½	A little cruor in vein.
8	32	Chronic dysentery .	" 8	8	A minute quantity of blood in vein.
9	19	Phthisis pul. .	" 11	1½	A very little fluid blood in vein, which speedily coagulated.
10	34	Empyema,—pneumathorax.	" 18	4	A little very dilute cruor in vein.
11	41	Phthisis P.,—pneumathorax	" 22	5½	Vein moderately distended with liquid blood, which, let out, presently coagulated.
12	38	Phthisis,—pneumathorax .	Nov. 7	20	Vein empty.
13	22	Pleuritis,—peritonitis .	" 9	12	Vein moderately distended with cruor which did not coagulate.
14	37	Pulmonary gangrene .	" 16	20	Vein empty; after removal of calvaria some air in the other jugular vein; none in carotid artery.
15	26	Phthisis pulmonalis .	Dec. 3	11	A little liquid blood from vein, which presently coagulated, and had a buffy coat.
16	27	Ascites; œdema of glottis; bronchitis	" 5	2	Vein distended; about 1lb. of blood flowed from it, when opened, coagulated feebly.
17	36	Aneurism of aorta,—Indurated lung	" 18	3	Vein moderately distended; coagulated when let out, without buffy coat.
18	61	Malignant tumour	" 29	20	A little blood from vein which did not coagulate.
19	33	Ruptured aneurism into trachea	Feb. 27	1½	Blood from vein presently coagulated without buffy coat.

TABLE II.
Cases in which the Carotid Artery and Jugular Vein were examined for Air, after the removal of the Calvaria.

No.	Age.	Fatal disease as determined by autopsy.	Time of death.	Time of examination (in hours) after death.	REMARKS.
1	18	Phthisis pulmonalis	Nov. 14	11	Several large bubbles of air in vein; two minute bubbles in artery.
2	20	Phthisis pulmonalis	Jan. 3	14	Air-bubbles in vein; none in artery.
3	21	Phthisis pulmonalis	" 9	33	A little air in vein and artery.
4	29	Phthisis pulmonalis	" 11	11	Some air in vein; very little in artery.
5	18	Empyema	" 11	33	A little air in vein and artery.
6	26	Phthisis pulmonalis	" 24	27	A little air in vein and artery.*
7	20	Phthisis pulmonalis	" 30	28	Some air in vein; none in artery.
8	20	Diabetes mellitus & phthisis pulmonalis	Feb. 13	27	A little air in vein; none in artery.

* In this instance the thorax and abdomen, as well as calvaria, were opened before the vessels were examined for air.

The results of the examination in the nineteen cases included in the First Table, were, as far as the artery was concerned, entirely negative ; not a single particle of air could be observed, in any instance, to escape from the vessel, opened under water. The artery, in every case, was collapsed ; flattened ; its upper and under surface in contact, from atmospheric pressure,* and either quite destitute of blood, or it contained only a very minute portion, in the space of an inch, never equal to a single drop. And, in the instances of the vein, with one exception, that of case No. 3, the result was similar, strongly confirming the absence of air.

The trials, the results of which are given in the Second Table, were made for the sake of comparison. I expected, that in every instance, air would be found in the artery as well as in the vein. The circumstance, that in three out of the eight cases in which the experiment was made, no air could be procured from the artery opened under water, — shews (to express the fact in popular language for the sake of brevity) how feeble is its power of suction ; and how very inadequate, on the principle of removal of pressure, to draw air from the blood, according to the idea involved in the hypothetical view of Professor Burdach.

As the results, which I have brought forward without selection, are perfectly in accordance with the

* When opened, either in the air or under water, in every instance, it immediately recovered its cylindrical form, air or water entering.

preceding observations of Harvey and of Prochaska, —may it not be fairly inferred, that their conclusion is the true one; that the arteries, commonly after death, do not contain air; and that when they differ from this their normal state, it must be presumed to be the effect of peculiar circumstances, requiring special investigation.

Whether the arteries, after death, usually contain something intermediate between fluid blood and elastic air, appears to me questionable in a very high degree. The distinguished physiologist last mentioned, appears to be of opinion, that they may, and that they partly owe to the vapour, which he infers they may contain, certain qualities peculiar to their vessels, particularly the manner in which they contract and shorten when divided.

IV.

OBSERVATIONS ON THE BLOOD AFTER DEATH.

FOR many years past, in the *post mortem* examinations of bodies, I have been in the habit of paying attention to the condition of the blood in the heart and great vessels, especially in relation to coagulation; and latterly, in conjunction with coagulation, to the effects of agitation on the blood in common air, as a test, whether or no it contain carbonic acid gas in excess, admitting of being extricated by this method.

I shall first give the general results of my observations, made in Malta, from the year 1828 to 1835, as an example of the variations in the appearances of the blood after death, and of their comparative frequency. For the sake of brevity I shall present them in a tabular form.

TABLE I.
Of the Appearances of the Blood after Death from various Diseases.

Year.	Total number of fatal cases examined.	Heart collapsed, containing either no blood or a very minute quantity.	Blood liquid; not coagulating on exposure to the air.	Partly liquid, partly coagulated, —the former not coagulating on exposure.	Partly liquid, partly coagulated, —the former coagulating on exposure.	Liquid and coagulating on exposure.	Coagulated and containing fibrinous concretions.	Coagulated, without fibrinous concretions, —the coagulum soft.	Grumous, not distinctly coagulated, without fibrinous concretions.	Liquid and frothy, —without any fibrinous concretions.	Blood florid.	Fibrinous concretions without any error.	Blood coagulated in both ventricles of heart, and broken up.	Coagulated and broken up in left ventricles, not in right.	Coagulated in right ventricle and broken up; left ventricle empty.	Cases in which the state of the blood was not noticed.
1828	28	—	2	2	1	1	21	—	—	—	—	—	—	—	—	—
1829	18	2	—	2	—	—	14	—	—	—	—	—	—	—	—	—
1830	36	—	1	1	—	—	18	—	2	1	1	—	—	1	—	11
1831	54	2	1	—	—	—	21	3	—	—	—	—	3	5	—	19
1832	29	2	2	—	—	—	8	1	—	—	—	—	1	—	1	14
1833	35	—	2	—	—	1	7	3	—	—	—	—	1	4	—	17
1834	40	—	1	—	1	2	14	2	—	—	—	1	—	1	—	17
1835	9	—	—	—	—	—	2	—	—	—	—	—	—	—	—	7
Total	249	6	9	3	6	4	105	9	2	1	1	1	5	11	1	85

The fatal cases, affording the subjects of these observations were, without exception, soldiers of our regiments, serving in Malta, composed of Englishmen, Scotch, and Irish.

In the cool season, the bodies were commonly inspected in from twelve to seventeen hours after death; in the hot season, in from three to twelve.

As the table is brought forward chiefly as an example, intended to shew how various is the condition of the blood after death, I shall restrict myself to a very few remarks on the different appearances of the blood noticed, and especially in connexion with the fatal diseases. To do justice to the subject, it would be necessary to enter into very minute details, and give a statement of particulars, which could be collected only by laborious research,—and which, at present, I am not at all prepared to offer.

1. The few instances in which the heart was found empty, or containing only a very minute quantity of blood, were chiefly cases of death from ruptured aneurism.

2. The instances in which the blood was found liquid and not coagulable on exposure to the air, the fatal event, in the majority of cases, was referrible to the respiratory organs, and was owing either to drowning, hanging, the fumes of a charcoal fire, or the effusion of blood into the bronchia or air-cells. The former occurred in a case of pneumonia; the latter in two cases of sudden death, from what Laennec has called pulmonary apoplexy.

The blood's liquidity in these instances was not a *post mortem* effect, resulting from putrefaction; this was clearly ascertained. It was owing either to a loss of the natural fibrin or lymph, on which the property of coagulation depends, or such an alteration in it, as to deprive the lymph of this quality. In one instance, a few days previous to death, some blood abstracted from the patient, exhibited in relation to coagulability its normal condition.

3. Relative to the condition of the blood noticed in the 4th and 5th column, as either liquid—coagulating on exposure, or partly liquid and coagulable, I believe, it depended (whether one or the other), on the time after death, when the *post mortem* examination was made: I shall have occasion further on, to bring forward examples in illustration of this remark.

4. Of the 164 cases in which the state of the blood was specially noticed, in 105 instances it was found coagulated, the coagulated blood mixed with fibrinous concretions, or coagulated lymph, the material of the buffy coat; and I believe it was found in the same state in the majority of the 85 cases, in which its condition was not specified.

5. The instances in which the blood was found coagulated and without fibrinous concretions (the coagula soft) were chiefly cases of tubercular phthisis. In the majority of the fatal cases, however, of this disease, fibrinous concretions occurred.

6. In one instance in which the blood was found

grumous, not distinctly coagulated, death was owing to confluent variola; the lungs were hepatized. In the other instance, the disease was returned debility; it was of a very obscure nature, and not connected with any well marked organic lesion. Whilst grumous blood was found in the cavities of the heart in this case, fibrinous concretions were met with in the great vessels.

7. In the solitary case in which the blood was found liquid and frothy, and did not coagulate on exposure, putrefaction was far advanced; and it is probable that the fibrin had become liquid after coagulation in consequence of putrefaction. In this instance all the parts in contact with blood were strongly stained by the colouring matter of the blood in a state of solution in the serum. The phenomenon of the red stain, as a *post mortem* effect, is, I believe, invariably connected with this condition of the serum, and which itself is connected more or less with putrefaction. I have never yet met with an exception to this remark; wherever I have found red serum, on microscopical examination, I have detected change in the form of the red particles, indicative more or less of decomposition.*

* Most animal textures, chemically considered, have a strong affinity for the colouring matter both of blood and bile,—and for the former especially when in solution, that is, separated from the blood-corpuscles, whether in water or serum. In a paper communicated to the Medico-Chirurgical Society of London, in 1818, and published in its Transactions,—I pointed out certain false appearances of inflammation, *post mortem* effects, connected with this

8. The single case, in which fibrinous concretion occurred in the heart without crassamentum or cruor, was one of dysentery.

9. The single case in which the blood was ob-quality. I then stated, "these false appearances are in many instances so exactly like the true, that I doubted if the most experienced anatomist would be able to distinguish between them:" adding, "that I had made the trial with medical friends by asking their opinion of parts coloured by immersion in blood, and, without the least hesitation they had pronounced the parts strongly inflamed:" afterwards in Malta, I continued the inquiry, and in 1828, made a series of experiments on the colouring power of blood and bile on the different textures of the body. The principal results were, that the stains of both are with difficulty removed by washing, and from several textures, even by maceration; and that by mere colour it is impossible to distinguish between the *post mortem* chemical effect,—and the effect, during life, of inflammation. The influence of temperature is of the first importance in relation to the production of bloody serum,—that is, serum holding in *solution* the colouring matter of the blood. At a temperature between 80° and 100°, the solution will take place in a few hours, and the staining will be produced,—the pseudo-inflammatory appearances will present themselves: on the contrary, at a temperature between 35° and 45°, the blood corpuscles will retain their colouring matter and remain unaltered, and the serum untinged for many days, and so long none of the false appearances of inflammation alluded to will be observable. Another point, of much importance, is the degree of tendency of the body after death to putrify. In some instances, even before death, there appears to be incipient putrefaction of the blood; and, in these cases, bloody serum is detected accompanied with staining of the parts with which it is in contact, though the *post mortem* examination may be made before the cadaver has had time to become cold. In other instances,—and they may occur at the same time,—the tendency to putrefaction is so very inconsiderable, that no indications of it are perceptible, even after twenty-four hours, and sometimes longer; and in these examples there is a corresponding absence of any false appearances of inflammation.

served florid, was an instance of *tabes mesenterica*, without tubercles in the lungs. The mesenteric and lumbar glands were vastly enlarged,—some of them the size of pigeons' eggs, and contained a curd-like albuminous matter; and tubercles containing a similar matter existed under the mucous coat of the transverse colon. The blood in this instance was coagulated; its florid hue was most strongly marked in the aorta.

The broken-up state of the coagulated blood referred to in the three last columns of the table, to the best of my knowledge, has not yet been described by any pathological writer. The appearance exhibited was as if one or both ventricles of the heart had continued to act after the coagulation of the blood had taken place within them. On no other idea can I account for the phenomenon. I never witnessed it in the blood-vessels, or even in the auricles, which perhaps may be considered in accordance with the supposition respecting its cause. Of the seventeen instances in which this condition of the blood was detected, death in the majority was owing to tubercular phthisis, viz. in 11; in two, the fatal disease was pneumonia, the lungs extensively hepatized; in one, abscess of the lung accompanied with gangrene; in one, chronic dysentery accompanied with abscess in the liver; and in one the death was from violence, a soldier in good health was shot through the head by a comrade. In

the majority of the instances, fibrinous concretions accompanied the broken-up crassamentum.

I shall now proceed, and briefly notice some later observations on the blood after death, made in the General Hospital, at Fort Pitt, during a period of nine months, viz. from January to September, 1838, in which attention, as before mentioned, was paid not only to the state of the blood in relation to coagulation, but also to its state in relation to carbonic acid gas.

The method employed was briefly the following: In every instance, after the removal of any fluid that might have been found in the pericardium, the great vessels within it were divided, the blood which flowed out was collected and put into a bottle and secured with a glass stopper for after-observation and experiment. The cavities of the heart and their contents were next examined. Sometimes the blood set apart was seen again the same day, to ascertain whether it had coagulated or remained liquid; but more frequently, not until the following morning, when also commonly it was subjected to agitation in atmospheric air included in a double-mouthed bottle provided with stop-cocks, to one of which was attached a bent tube for communication with a pneumatic trough.

I shall give the results in a tabular form individually, with the age and fatal disease, the day of the month of its termination, the interval between the time of death and *post mortem* examination, and

between the latter and the examination of the blood, —circumstances without which little value can be attached to the record; and under the head of Remarks, will be appended the organic lesions discovered, of most importance, characteristic of the disease, with occasional notices of lesions of rare occurrence.

I may premise further, that in using, for the sake of brevity, the term “cruor,” I have not employed it to designate merely a mixture of serum and red particles, but a mixture of the two in which there may be some coagulable lymph, to be determined by trial. The terms “crassamentum” and “clot” I have used synonymously, in their ordinary acceptation, as applied to the blood. The term, “fibrinous concretion,” I have employed to designate masses of lymph or fibrin, in a coagulated state, such as used to be called polypi.

TABLE II.

Of the Condition of the Blood after Death from various Diseases, and its Changes.

No.	Age.	Fatal disease (determined by dissection.)	Time of death.	Post mort. in specimen (hrs. after death.)	General condition of the blood in the heart.	After condition of the blood set apart.	Results of agitation of the blood or cruor.	Remarks relative to morbid appearances.
1	19	Pericarditis	Jan. 20	16	Pretty much cruor and a little clot in right cavities of heart: no fibrinous concretions.	-	Agitated immediately: absorbed a little air: and again the following day.	Pericardium nearly an inch thick from deposition of lymph: contained 20 ounces of purulent serum. Granular tubercles in the lungs.
2	29	Phthisis pulmonalis	" 26	18	About three ounces of cruor and dark crassamentum in right cavities of heart.	Two hours after, the cruor found coagulated feebly.	Immediately after coagulation gave off some air on agitation. On 1st Feb. had an offensive smell: and on agitation absorbed a little air.	The lungs abounded in tubercles and vomicae: the left pleura contained 46 ounces of purulent serum, which, on agitation, gave off a good deal of air.
3	22	Rubeola	Feb. 3	28	Much dark liquid blood in right cavities of heart.	-	Agitated about two hours after gave off some air: agitated a second time, immediately there was slight absorption: rubbed with quick lime, a slight ammoniacal odour.	Bronchia dark red: engorgement of pulmonary vessels, and some ecchymosis in air-cells: death on 4th day.
4	18	Phthisis pulmonalis	" 19	29	Much cruor with fibrinous concretions in right cavities.	Six ounces of cruor collected, found liquid 20 hours after.	Agitated 20 hours after, gave off about half cubic inch of air.	Lungs abounding in tubercles and small vomicae: partially hepatized: partially oedematous: a portion of large vein in right lung to the extent of about half an inch plugged up with firm fibrin.

TABLE II.—Continued.

No.	Age.	Fatal disease (determined by dissection.)	Time of death.	Post mort. in specimen (hrs. after death.	General condition of the blood in the heart.	After condition of the blood set apart.	Results of agitation of the blood or cruor.	Remarks relative to morbid appearances.
519		Bronchitis	Feb. 23	33	Much cruor and fibrinous concretions in right cavities: the former of sp. gr. 1045.	Cruor did not coagulate.	Cruor agitated on 28th Feb. gave off no air: three days later it gave off some air: it had then a putrid smell.	The bronchia smeared with pus: an ulcer on upper surface of epiglottis: two small clusters of granular tubercles in lower part of superior lobe of right lung.
620		Phthisis pulmonalis	Mar. 22	30	A good deal of cruor and fibrinous concretions in right cavities.	Six ounces of cruor collected, on following day slightly coagulated.	On agitation, after having been broken up, a very slight absorption: agitated with carbonic acid, it absorbed 116 per cent.	Cavities and tubercles in both lungs.
724		Chronic pericarditis	" 30	14	Some cruor, soft crassamentum, and a little fibrinous concretion in right cavities.	The following morning the cruor was found feebly coagulated.	On agitation pretty much air disengaged: and also from the serum.	Seven pounds of serum in cavity of abdomen: thickening of the peritoneal covering of most of the viscera: a few clustered granular tubercles in lungs: tubercles in kidneys, prostate and testes: and loose putty-like matter in left vesicula seminalis.
870		Chronic peritonitis, with abscess and gangrene of lungs	Apr. 13	48	Only about three ounces of blood (crassamentum) from the heart without fibrinous concretion.	- - -	Clot broken up and agitated: there was a slight absorption of air.	A small gangrenous abscess in right lung: no tubercles in either lung: 16 pints of serum in cavity of abdomen: some pus in cavity of pelvis. Liver weighed only 2lbs.: liver and pancreas unusually firm. The serum agitated the following day gave off a good deal of air: the patient for many years had laboured under amentia.

959	"	May	3	26	Cruor and soft coagula in right cavities of heart.	Three ounces of cruor collected, the following morning jellied.	On agitation, after having been broken up, gave off a good deal of air.	Blood effused on left hemisphere of cerebrum on the arachnoid membrane: fornix soft. Inferior lobe of left lung œdematous, contained two cavities in which a puriform fluid: no tubercles. A patient in Lunatic Asylum labouring under amentia.
1020	Phthisis pulmonalis	"	5	22	Pretty much cruor and fibrinous concretions in cavities of heart.	The cruor slightly coagulated the following morning.	Broken up, a good deal of air given off on agitation.	Many clustered granular tubercles, and two or three small cavities in superior lobe of left lung: several clusters of granular tubercles in superior lobe of right lung: right lung extensively hepatized: 8 ounces of purulent serum in right pleura: 3 ounces in left: 2 ounces in pericardium.
1121	Œdema glottidis	"	6	21	A good deal of dark liquid blood in the large vessels, and in right cavities of heart: a small portion of fibrin in right ventricle.	After three hours continued liquid	On agitation gave off a good deal of air.	Unexpected and almost sudden death from œdema of glottis, margin of epiglottis and sacculi laryngis: the uvula was also œdematous.
1230	Bronchitis, chronic phthisis pulmonalis	"	17	48	A good deal of cruor and fibrinous concretions in cavities of heart.	-	On agitation immediately gave off some air.	Bronchia red, and contained muco-purulent fluid: under-surface of epiglottis superficially ulcerated: an ulcer at base of right arytenoid cartilage: numerous granular tubercles in both lungs: a small vomica in superior lobe of right, and three in the superior lobe of left lung. The body little emaciated.
1329	Phthisis pulmonalis	"	17	45	Some cruor and fibrinous concretions in right cavities of heart.	-	On agitation immediately gave off no air: there was no absorption.	Lungs abounded in tubercles, and contained several cavities: large intestines severely ulcerated. Died suddenly in the act of speaking.
1431	Phthisis pulmonalis	"	25	27	A good deal of coagulated blood, cruor, and fibrinous concretions in right cavities of heart.	The cruor put aside was liquid the following morning.	On agitation gave off a little air.	Large cavities and numerous tubercles in the lungs: lungs partially hepatized: in the right ventricle of heart were two masses of fibrin closely adhering, in each of which was a puriloid fluid, consisting (as it appeared under the microscope) of a few blood discs, globules very like those of pus, and of other particles less regular, and of various sizes.

TABLE II.—Continued.

No.	Age.	Fatal disease (determined by dissection.)	Time of death.	Post mort. in- spection (hrs. after death.)	General condition of the blood in the heart.	After condition of the blood set apart.	Results of agitation of the blood or cruor.	Remarks relative to morbid appearances.
1531		Pneumonia	May 26	26	A good deal of cruor, coagulated blood and fibrinous connections in cavities of heart.	Cruor liquid the next morning.	On agitation gave off some air.	Some oedema of left lung, weight three quarters of a pound: right lung extensively hepatized, of a fawn colour: it weighed four pounds: a slight deposition of lithate of soda on cartilages of tibiæ and of first joint of great toes. It was reported that he had been subject to rheumatism.
1628		Phthisis pulmonalis	June 1	12	Some crassamentum,—fibrinous concretions and a good deal of cruor in right cavities of heart.	Two hours after, the cruor was found jellied.	After twenty-four hours the coagulum had contracted: serum had separated: broken up and agitated, some air given off.	Cavities and numerous tubercles in lungs: severe ulceration of larynx: in a mass of fibrin in the right ventricle was a collection of transparent serum: the mass was firmest externally.
1734		Aneurism	July 21	26	Six ounces of thin cruor in right pleura, with a very small portion of clot.	The cruor did not coagulate.	Agitated twenty-four hours after, gave off a very little air.	A false aneurism close to celiac artery, with two circular openings from aorta within half an inch of each other: the sac having burst, a large quantity of blood was poured out and coagulated in omentum, &c.: a portion entered the posterior mediastinum and ruptured it.
1839		Phthisis pulmonalis	" 24	26	A good deal of dark cruor and fibrinous concretions in right cavities of heart: a mass of fibrin in left ventricle.	The cruor (six ounces) was found jellied the following day.	Broken up and agitated it gave off a little air.	Several cavities in lungs, and numerous tubercles with some hepatization.

1930	"	"	29	27	Some cruor and fibrinous concretions in right cavities of heart and in left auricle.	After twenty-four hours cruor slightly jellied.	On agitation gave off a very little air.	Tubercles abounded in lungs with cavities, partial oedema and partial induration.
2024	"	"	31	37	Some cruor, coagulated blood and fibrinous concretions in right cavities of heart and in left auricle.	- - -	Agitated immediately, pretty much air disengaged.	Right lung weighed three and half pounds, partially hepatized, contained cavities and abounded in tubercles: left lung similarly diseased, in a minor degree.
21	Hypertrophy of heart	Aug.	1	32	A good deal of cruor in right cavities of heart and some clot.	- - -	Agitated immediately, gave off a good deal of air.	Heart nearly triple its natural size: ventricles most enlarged, especially left: aorta much dilated and diseased, an aneurismal pouch at its base: albuminous matter of the appearance and consistence of soft putty in left tunica vaginalis testis, left vesicula seminalis, in cavities in substance of kidneys, in ureter of same side, and in right vas deferens: the left ureter was in several places impervious: partial oedema of lungs: no tubercles.
2233	Phthisis pulmonalis	"	5	4	Much liquid blood in cavities of heart: some crassamentum: no fibrinous concretions.	Sixty-three ounces collected, which coagulated in a few minutes.	Broken up the following day, on agitation it gave off pretty much air.	Large excavations and numerous tubercles in lungs: death probably from suffocation, a vomica bursting and pouring its contents into bronchia.
2346	Hydrothorax	"	4	29	A little cruor and coagulated blood in right cavities of heart.	About one ounce only could be collected: the following day cruor coagulated.	Quantity too small for trial for air.	54 ounces of serum in right pleura, with flakes of lymph: 18 ounces in left pleura: 5 ounces in pericardium, without deposition of lymph: the serum agitated gave off pretty much air: granular tubercles in lungs and in right pleura: 32 ounces of serum in cavity of abdomen: were all the blood in the body collected, it would not have exceeded, it is probable, 4 ounces.
2422	Phthisis pulmonalis	"	9	12	Pretty much cruor and fibrinous concretions in right cavities of heart: very little in left.	Cruor did not coagulate.	The following day, on agitation, gave off pretty much air.	27 ounces of sero-purulent fluid in left pleura: lined with false membrane: cavities and tubercles in both lungs: a mass of softened fibrin in right ventricle, which it may be inferred existed before death: a mass of firm fibrin in left ventricle of heart.

TABLE II.—Continued.

No.	Fatal disease (determined by dissection.)	Time of death.	Post mort. in- specion (hrs. after death.)	General condition of the blood in the heart.	After condition of the blood set apart.	Results of agitation of the blood or cruor.	Remarks relative to morbid appearances.
25	25 Lumbar abscess	Aug. 22	14	A good deal of cruor in right cavities of heart; a little soft fibrinous concretion.	Cruor found coa- gulated the fol- lowing morning.	Broken up and agi- tated, gave off pretty much air.	Pus in bronchia of left lung: a portion of its inferior lobe hepatized, another portion cedematous: no tu- bercles: lumbar vertebræ and right os ilii exten- sively carious.
26	28 Pneumonia	" 5	19	A good deal of cruor, crassamentum and fi- brinous concretions in heart.	Two hours after re- moval, cruor co- agulated and covered with a thin buffy coat.	Broken up and agi- tated the following morning, gave off no air: nor absorbed any.	Right lung weighed 3½ lbs.: its superior lobe com- pletely hepatized: some of its vessels filled with lymph: left lung in part cedematous: no tubercles. The liver very large, not fatty, weighed 13½ lbs.: 8 oz. of serum of sp. grav. 1010, without any lymph.
27	47 Phthisis pulmonalis	" 14	24	Some cruor, a good deal of crassamentum and fibrinous concre- tions in right cavities of heart,—some fib- rinous concretions in left ventricle.	Cruor, the follow- ing day found jellied.	Broken up and agi- tated, a little gas was disengaged.	Both lungs contained numerous tubercles and some cavities: arch of aorta considerably enlarged: two masses of fibrin adhered to it internally mid-way, about the size of filberts, softening but not poul- taceous.
28	35 Medullary Tumour	" 17	5	Some cruor and a good deal of crassamentum in right cavities of heart.	The cruor, the fol- lowing morning, pretty firmly co- agulated.	Broken up and agi- tated, it gave off a little air.	A considerable portion of the left side of face de- stroyed, with the greater part of corresponding maxillary bones: a tumour about the size of a lemon over the left parotid gland, its central part soft, red- dish and blood-shot: composed of particles which under the microscope appeared to be of different sizes; some of them globular.—Viscera tolerably sound.
29	40 Dysentery chronica	" 13	4½	Some cruor and fibrin- ous concretions in right cavities of heart.	After 2 hours, the cruor was found coagulated, & a slight buffy coat on it.	Broken up and agi- tated the following day, no air given off: a very slight absorp- tion.	Large intestines severely ulcerated: a mass of fibrin in longitudinal sinus softening internally.

3026	Phthisis pulmonalis	"	19	16	Some cruor in right cavities of heart,—no clot or fibrinous concretion.	Cruor liquid the following day.	On agitation did not give off any air: no apparent absorption.	Vomicae and numerous tubercles in lungs: appendicula vermiformis distended with purulent fluid: ulcerated: its opening into caecum perfectly closed by adhesion, (an example of mucous surfaces adhering by the effusion of lymph.)
3122	"	"	19	18	Some cruor and fibrinous concretions in right cavities of heart.	The following morning the cruor slightly jellied.	Agitated, gave off a good deal of air.	Numerous tubercles in lungs, and many minute vomicae: disease of bone of right upper extremity: portion of ulna necrosed: cartilage of head of humerus and glenoid cavity destroyed: pus in capsule.
3240	"	"	22	8½	Some cruor,—a considerable quantity of crassamentum and some fibrinous concretions in right cavities of heart.	The cruor after two hours jellied.	Agitated, it gave off a little air.	Left lung very small, many tubercles in it, and a cavity communicating with pleura: left pleura contained 57 oz. of turbid serum, and a good deal of air: vomicae and tubercles in right lung: the serum from pleura on agitation gave off a good deal of air.
3341	"	"	26	5	Some cruor,—and fibrinous concretion and a good deal of crassamentum in right cavities of heart; little in left.	The next day the cruor was found coagulated.	Broken up and agitated it gave off no air, and did not apparently absorb any	Vomicae, and tubercles in both lungs: left femoral and iliac vein with their branches obstructed by coagulated blood and fibrinous concretions,—the latter in many places softening, and in one or two reduced to a pus-like state.
3420	Ascitis	"	28	27	No blood in heart,—blood from jugular veins liquid.	The following day found liquid.	On agitation it gave off pretty much air.	7 pints of turbid serum in right pleura: 84 ounces of clear serum in cavity of abdomen: some pus in pericardium with great thickening, partial adhesion: a few crude tubercles in lungs, and one small cavity: the serum from abdomen was of sp. gr. 1018: the following day, it had deposited a very little lymph: on agitation, it gave off a good deal of air.
3545	Medullary tumour	Sept. 1	21		A good deal of soft crassamentum, some fibrinous concretions and a very little cruor in heart.	The cruor did not coagulate.	The quantity too small for trial for air.	Masses of cerebriform tumor in left arm, accompanied with much disorganization of soft parts; similar disease in substance of left kidney: the lower part of vena cava, the left iliac and femoral veins and their branches obstructed by coagulated blood and fibrinous concretions, the latter in many places softened and puriform.

The instances in the first Table were strongly indicative of the variable quality of the blood after death, from disease. The particular instances just given are as strongly corroborative of the same. The variable quality, as far as appreciable by the senses, seems to depend on the lymph, which as regards coagulability, has appeared wonderfully little constant ; in some instances retaining its power of coagulation more than 26 hours after death,—in others, coagulating immediately after the fatal event,—and in some, previous to that event. In a large number of the cases, the lymph appears to have existed in the blood possessed of the quality in question in different degrees. This is clearly to be inferred from the results of the *post mortem* inspections, viz. from finding at the same time, and in the same body, in the cavities of the heart, liquid coagulable cruor, soft crassamentum, and firm fibrinous concretions.

The fibrinous concretions, which are so frequently found in the heart after death, are far from being devoid of interest. Their appearance, both as regards form and consistence, is very various. They are also very various in relation to the manner in which they are attached to the heart. I have mentioned one instance in which serum was collected in the interior of a mass ; the inner part was soft, the outer surface firm. Some masses seem to have a certain regularity of structure ; some have the appearance of being inflamed, being blood-shot from

entangled red particles of blood ; some exhibit the appearance of abscess, and contain as it were in a sac a puriform matter, generated by a peculiar process of softening. Generally they tend to give the idea of the operation of an organizing principle,—a *nisus formativus* more or less active until putrefaction commence, by which the animal matter is converted into common matter.

The different varieties of fibrinous concretions are admirably illustrative of certain morbid appearances, especially false membranes, bands of adhesion, the state of the cellular tissue in different kinds of œdema and induration, and, I may add, hepatization of the lung with and without softening, and a similar state of the spleen, and perhaps tubercles and their softening. When I have examined microscopically the matter yielded by the lung which has been hepatized, undergoing softening, I have found it very similar to the matter into which a fibrinous concretion is converted, whether in the ventricles of the heart or in a vein, composed of particles of different sizes, most of them approaching the spherical in form, and some of them very like pus globules ; and I have found the same kind of matter in softened spleen, and also in softened tubercular matter.

No subject, probably, in pathology is more deserving of careful investigation than lymph, in connection with its varieties, supposing either that there are many different species of it, or that it is liable

to sudden and great changes of quality. Relative to the former, take for instance the phlebolite in its different stages of induration, from soft lymph becoming of cartilaginous firmness and appearance; the fibrinous layers deposited in an aneurismal sac, resisting change occasionally for years; and fibrinous concretions which are formed in the large veins, especially in phthisis, which so soon soften, and in part almost liquefy;—how great is the diversity,—how difficult, in the present state of our knowledge, is the explanation.

Nor is the liquid cruor, or the blood remaining either in part or altogether liquid after death,—whether for a definite or indefinite time,—without interest and importance, especially as regards pathological research, and the distinguishing between the effects of disease and *post mortem* effects in the inspection of bodies. As long as the blood is liquid after death, it must necessarily observe the laws to which fluids are subject under similar circumstances; will accumulate in the dependent parts, and where there is least resistance; and should there be considerable pressure exerted from the disengagement of air in the stomach and intestines, it will appear as if injected in the organs removed from such pressure, whether included in unyielding parieties, as the brain,—subject to occupy less volume from greater reduction of temperature after death, than its solid, and during life cooler case,—or contained in yielding envelopes, as the lungs in the pleuræ and other parts

of the body, generally in the common integuments, and in their peculiar capsules. In a considerable number of examples given in the table, cruor was found in the heart more than twenty-four hours after death. This cruor, in producing false appearances of inflammation and congestion, would act much in the same manner as liquid blood, and in estimating the phenomena which present on dissection, much the same allowance should be made for it,—or serious errors will be unavoidable.*

From the experiments given in the table, on the agitation in air of the blood collected after death, it would appear that in the greater number of instances, gas was disengaged. In several instances the air thus liberated was tried, and was found to be car-

* Dr. Yellowly in his valuable paper entitled "Observations on the Vascular Appearance in the Stomach, which is frequently mistaken for Inflammation of that Organ," published in the Transactions of the Medico-Chirurgical Society for 1813, has forcibly called the attention of medical men to errors such as these above alluded to. His remarks on the stomach are, I apprehend, applicable to every other vascular organ; and I am not aware of any phenomena pointed out by him, that are not referrible to the circumstances specified above. The long time that a portion of blood may remain liquid in the body after death, does not appear to have been known by him; indeed previous to the observations which I have given in proof of it, I believe it had not been even suspected.

The false appearances of inflammation described by Dr. Yellowly, are not to be confounded with those, the effects of mere staining: in one instance the vessels merely contain blood, or cruor, as if injected, and which may be washed out, leaving the part colourless: in the other instance, the colouring matter is chemically combined with the vessel or texture, and cannot be removed by simple ablution.

bonic acid gas,—proving that there was an excess of this acid in the blood. This excess, I am disposed to infer, especially from the results of experiments which I have made on healthy blood, as already mentioned, is the effect of disease, and is particularly apt to take place, as before remarked, in the act of dying, when the powers of secretion seem to be arrested, and carbonic acid probably ceases to be eliminated in the lungs. In accordance with this, is the uniform dark colour of the blood, which is usually observed after death, and equally in the left and in the right ventricle of the heart; and also the little suffering, fortunately, commonly witnessed in dying,—stupor, or coma, usually preceding and ushering in the fatal event. From the results referred to, and from a few which I have obtained in operating on blood, taken from persons labouring under disease, I am disposed to think that carbonic acid acts a very important part in the economy of life, and is connected, when in excess, if not with the production of particular diseases, at least with their modification and progress, and the production of certain symptoms,—and that the careful examination of the blood, in relation to this acid, is an inquiry much wanted, and is likely to reward richly those who engage in it. At present there is a great difference of opinion, derived from experiment, respecting the carbonic acid in the blood: some inquirers, as Dr. Stevens, to whom belongs the merit of having opened this path of research,

considering it, in the healthiest condition, always in excess, and capable of being liberated from venous blood, and even from arterial, by the physical agencies in play in respiration; whilst others, as already pointed out, take an opposite view, and deny that any free acid exists in the blood, in its healthy normal state, or can be separated from the blood excepting by processes, in which it is formed from its elements. This discrepancy, the inquiry I have mentioned will probably explain or reconcile. It will elucidate also, probably, some other obscure points which might be mentioned, especially where it extended to other fluids—as the urine and bile. I may mention, incidentally, that whenever I have obtained indications of the disengagement of carbonic acid from the blood after death, they have been afforded by the fluids spoken of, in a very limited number of trials, and also by any effused serous fluid, collected at the same time and subjected to the same test.

In most of the cases brought forward in the second table, the blood was microscopically examined, using the achromatic object-glass of one-eighth of an inch focal distance, before-mentioned. I shall notice briefly some of the results.

The blood-corpuscles were seldom found perfectly uniform either in size or appearance; slight differences were perceptible. In some instances they appeared thinner than usual and more expanded; in some, of perfectly regular outline; in others, the discs had an uneven indented margin, and a puckered sur-

face. In some, in the same specimen, the particles exhibited these variations. In most cases in which gas was disengaged from the blood on agitation in common air, the blood-discs, under the microscope, appeared to have minute globules attached to them, and very often, instead of gliding on their flat surface, or with a revolving motion, they moved in the current, passing under the object-glass in a perpendicular position, as if suspended in this manner by particles of air adhering to their upper edge.

Lastly, in every instance, without exception, in which, during life, there were purulent discharges, or purulent collections were detected after death, globules, like pus-globules, were visible, under the microscope, on dilution of the blood with water.

I make mention of these globules in a guarded manner, from the circumstance that I have occasionally seen similar ones, in instances in which no suppuration preceded death, and also in the blood of the sheep in perfect health. Thus, in the month of April, of the present year (1839), on examining, microscopically, the blood of six different sheep killed the same day,—without exception, particles resembling pus-globules were detected, using the same method as that just described. In these and in the former instances of the absence of the suppurative process, however, it deserves to be remarked, that the particles in question were fewer in number than in the cases first referred to. Till the exact nature of the pus-like globules is determined, their

pathological influence must necessarily be involved in some degree of doubt.

I was led to institute these trials, in search of pus in the blood, from some observations made by Mr. Gulliver, who, I believe, was the first to detect this substance, or its similitude, in the circulation.*

* The observations I refer to, are contained in a very original and ingenious paper by this gentleman, which was read before the Royal Society last spring, and which has since been published under the title of "Researches on Suppuration," in the Philosophical Magazine, for September, 1838.

This discovery (supposing that the particles are true pus-globules) which I attribute to him, of course rests on precise microscopical observation, and is not to be confounded with the imagined detection of pus in the veins, and its inferred existence in the blood, in instances designated phlebitis, the majority of which, hitherto described, I am disposed to believe, were examples not of true inflammation of vein, but merely of obstruction from the formation of a fibrinous coagulum, with a tendency to soften and assume a puriloid character.

V.

AN ACCOUNT OF SOME EXPERIMENTS TO ENDEAVOUR
TO DETERMINE WHETHER OR NO, FREE CARBONIC
ACID EXISTS IN THE URINE OF MAN IN HEALTH.

M. PROUST, many years ago, maintained that the human urine contains carbonic acid; and M. Vogel, and Mr. Brande, have expressed themselves to the same effect. The first mentioned inquirer was of opinion that this acid exists in excess in the secretion of the kidneys.—“Nos urines (his words are) en sont surchargées.”* The last, Mr. Brande, has not adopted this conclusion; his own is, that the quantity is variable; he says, “When placed under the receiver of an air-pump during exhaustion, the gas escapes, sometimes copiously, but at other times in minute quantities only.”†

M. Proust drew his inference from the appear-

* Annales de Chimie et de Physique, tom. xiv. p. 258.

† Manual of Chemistry, vol. iii. p. 192, 2nd edition. Mr. Brande, in the last edition of his work, that of 1838, in mentioning the ingredients of healthy urine, includes carbonic acid.

ance of the froth which rises in boiling urine, and from the precipitate procured by adding lime to the fluid. His statement on the subject is very brief;—the following is the whole of it. “Au moment où elles (nos urines) commencent à bouillir, on les voit se couvrir d’une écume blanche volumineuse qui en est gonflée, comme je m’en suis assuré, en recueillant une certaine quantité dans une cloche. Si, en outre, on y jette de la chaux en poudre, la plus grande partie se change en carbonate.”

The experiments which I have instituted myself on this subject, have afforded results which are not reconcileable either with those of M. Proust, or of Mr. Brande.

On the first it appears to me unnecessary to offer any remarks, as the methods employed are of very doubtful accuracy. In some carefully conducted trials, in which lime-water was added to fresh urine in close vessels, I could detect no carbonate of lime in the precipitate recently examined and not collected on a filtre. If, however, many hours were allowed to elapse, then carbonic acid was extricated from the precipitate on the addition of dilute muriatic acid.

On the effects of the air-pump on the urine, which may be considered an unobjectionable test of the presence of a free gas, or simply absorbed as the carbonic acid in this fluid has been supposed to be, I have made very many experiments, both in Corfu in 1828, and during the last twelve months in this

country. In the Ionian Islands I used an air-pump, in good order, but not of great exhausting power. Recently the instrument I have employed has been of the best construction, the same as that referred to at page 155 of this volume.

In both series of experiments, [taking proper precautions as regards temperature, and the exclusion as much as possible of atmospheric air by receiving the urine, as voided, into vials scrupulously clean, and from which distilled water deprived of air by the air-pump had been the instant before emptied,] the results obtained were on the whole clear and satisfactory.

In the majority of instances, when the exhaustion was carried as far as possible, the urine cooled below 60° remained perfectly tranquil,—indicating the total absence of any gas.

In some comparatively few instances, air was disengaged. To determine the nature of the disengaged air, a little solution of pure potash was added to the urine, and the exhaustion was repeated. In every example that recourse was had to this test, the disengagement of air continued, clearly shewing that it was not carbonic acid; and rendering it probable that it was entangled atmospheric air, introduced in the act of filling the vial,—it being difficult always to prevent the production of froth in this act, in a slight degree.

In illustration, I may mention four trials of the same urine, made under circumstances, apparently

as nearly alike as possible,—having been received as the bladder was emptied into four vials, just emptied of distilled water, and placed immediately to cool side by side in the open air, at a temperature of about 50° . In about half an hour, they had fallen to about 54° . Then they were in succession placed under the receiver, and the exhaustion effected. No. 1, boiled up very much. No. 2, remained perfectly tranquil. No. 3, gave off a very few particles of air. No. 4, remained perfectly tranquil. In these instances, I did not test No. 1, and 3, by potash,—the other results seeming to demonstrate that the air disengaged was adventitious, and derived from the atmosphere.

The experiments which I have made have been numerous,—on the urine of the same individual at different hours for many days consecutively, and also at intervals at different seasons of the year;—and further on the urine of different individuals under various circumstances. The general result has been as I have stated, negative, and the more uniformly and perfectly so, the more accuracy has been observed in manipulating,—the greater the precautions taken to prevent the admission of atmospheric air.

From what I have witnessed generally in conducting the inquiry, I am strongly impressed with the conviction, that in a state of health, no free carbonic acid passes from the blood through the kidneys, or is conveyed dissolved in their secretion, and consequently that these organs do not perform

a part in the economy similar to that of the lungs, in separating this acid gas from the blood in circulation.

M. Proust in remarking on the results which he obtained, those already referred to,—seeming to him to shew that carbonic acid gas abounds in the urine, says, “we ought not to be surprised at it, as this gas is eliminated from the system in various other ways.” “Cet acide, considéré comme éjection ne faut pas nous surprendre. Nous en perdons par la transpiration par l’exhalation pulmonaire, et il fait partie des gaz que la digestion rejette par d’autres voies.”

This, fairly considered, does not strengthen his conclusion; but is perfectly consistent with the opposite one,—that the kidneys do not belong to its regular channel of outlet. If carbonic acid is capable of passing through the kidneys in the state under consideration, it appeared to me probable that it might be detected in the urine after drinking largely of a fluid abounding in it. I have made two trials with a view to determine this. At Corfu the agreeable effervescing mineral water of Cilli in Styria, which contains an enormous quantity of carbonic acid, and some carbonate of lime, was drunk fasting, a pint at a draught; and recently in this country about the same quantity of soda water in the act of effervescing was used. The bladder had been previously emptied. The urine on both occasions, collected within an hour after, was found

perfectly tranquil under the exhausted receiver ; not a particle of air was disengaged from it, the exhaustion being as complete as possible.

If the carbonic acid, when taken into the stomach in large quantity, is not conveyed into the bladder in the fluid which passes off so rapidly by the kidneys, but is separated from it, either altogether in the *primæ viæ*, or in part in the blood, it seems highly improbable that these organs, ever, excepting when in a diseased state, should separate this gas from the blood ; and when the composition of the blood is taken into account, and the state in which the alkali it contains appears to exist in it, — namely, as I have endeavoured to show, in that of sesquicarbonate,—this conclusion is in a great degree strengthened ; and it appears to me also much corroborated by keeping in view the peculiar function and structure of the lungs as an exhaling organ acting exposed to rarefied air, and as it were in a partial vacuum, compared with the kidneys, so differently situated and constituted, and having a function to perform so different.

The principal difficulty I have experienced in coming to the conclusion which I have been obliged to draw from my own experiments has been to account for M. Brande's opposite results. I can only reconcile them with those which I have obtained, by supposing that he did not give his special attention to the subject ; and that the trials he made of the effects of exhaustion were conducted without those

precautions being taken which I have adverted to, and which troublesome experience has taught me are absolutely necessary to guard against fallacy in the inquiry.

I have mentioned as a presumption, that in disease, the human urine may contain carbonic acid. But whether it does in reality I apprehend remains to be proved. Hitherto, in the trials which I have made of this fluid in patients in hospital, I have not detected it. My experiments on it however under the influence of disease, have been limited, not sufficient to allow me to come to any satisfactory conclusion. After death, I have detected it in the urine, the instant it was taken from the bladder; in these cases it may have been generated in the bladder from a change in the urea, which is commonly secreted to the last, and which, as is well known, in its unpurified state, is peculiarly apt to decompose and produce carbonate of ammonia, which, were any other acid present, would necessarily afford carbonic acid gas.

Whether or no azote, or any other gas is ever contained free in the urine, is a question which perhaps cannot at present be answered in a satisfactory manner, and which can be determined only by carefully conducted experiments specially directed to the investigation of the doubtful points.

VI.

NOTICE OF SOME ADDITIONAL EXPERIMENTS WITH
THE AIR-PUMP, ON SOME OF THE OTHER SECRE-
TIONS, AND ON SOME OF THE SOLID PARTS OF
THE BODY.

1.—*Of Milk.*

I AM not aware of any attempts hitherto made to endeavour to determine, whether milk contains free carbonic acid, or that any opinion even has been hazarded on the subject.

Both at Corfu, and recently in this country, I have subjected milk to the air-pump. The results of the best conducted experiments have been negative. At Corfu, in 1828, I made trial of woman's milk, and of cow's milk; only a very few bubbles of air were extricated on exhaustion; no more than it might be fairly inferred were entangled in the fluid as it was expressed from the nipple into the receiving vessel. In this country I have experimented only on cow's milk. When precaution was taken to prevent frothing, as by expressing the milk from the nipple, immersed in water deprived of air by the air-pump, in the manner analogous to that used in the

similar trials on the blood,—then, on subjecting it to exhaustion when cooled, it remained perfectly tranquil, not a particle of air was disengaged, even when the vacuum was as complete as it could be made.

At Corfu, I made also some experiments on the new laid egg of the common fowl, and with the same results; neither its white, nor its yolk afforded any air *in vacuo*. The trials were made on the white and yolk apart under water.*

Are not these negative results in the instance of milk and the egg, such as might be expected, *à priori*, whether we take into consideration their source, or their composition, or the objects for which they are destined? And, may they not in part tend to account for some of the peculiar qualities of each in their state of perfect newness, and for the superior wholesomeness of the former, when, as by nature it was intended, it is drawn by the infant or young animal direct from the breast of the mother?†

* When the whole egg was immersed in water, and subjected to the air-pump, then indeed it gave off some air, as might be expected; adhering bubbles were detached from the outer surface of the shell generally; a current of bubbles, for several minutes, was poured out from two or three conspicuous pores in the large end of the egg—air, there is little doubt, which had entered through the same pores, during the cooling of the egg, and its consequent contraction,—which I infer from not finding any air in this part of the egg, when it has been put into water the instant it has been laid, and examined under water.

† The air which milk rapidly absorbs, on exposure to the atmosphere, and which is promoted by the frothing in milking, and in the agitation it is commonly subjected to afterwards in various ways,

2.—*Of Bile.*

Recently I have subjected, with all possible care, the bile of the ox, the sheep, and the pig, to the air-pump. As soon as the animal was killed, a ligature was applied to the cystic duct, and the gall-bladder was dissected out. In the instance of the ox, the bladder was immersed in water, and brought to me, and the bile, within a quarter of an hour, was received into water that had been deprived of air; in that of the sheep the transfer was made on the spot; but in that of the pig it was delayed about fourteen hours. In each instance, no air could be extricated by exhaustion; the results were most satisfactory,—not a particle appeared.

On the bile of man, I have recently made only one trial, conducted in the same manner as the preceding; it was procured from a body, examined twenty hours after death, in the beginning of May, that had died of tubercular phthisis, complicated with paraplegia, occasioned by the growth of a tumour of the medullary kind in the lower part of the spinal canal. Although a cavity in the lungs was in a gangrenous state, the blood bore no marks of incipient putrefaction; and not a particle of air was disengaged from the bile *in vacuo*.

In Corfu, I made a similar trial of cystic bile, in two other fatal cases, both of chronic dysentery, with

conduces powerfully to its fermentation—to the formation of carbonic acid and vinegar, and the separation of its curd and cream.

the like result ; one examined 13 hours, the other, 20 hours after death.

The deduction from these results is obvious : and, in relation to carbonic acid, as bile is an alkaline fluid, they are such as might be expected *à priori*.

At page 211, I have mentioned, that on trial of bile, collected after death, in instances of disease, by agitation with common air, gas was frequently disengaged, which it was supposed was carbonic acid gas. In these examples, it is probable, that the disengagement witnessed, was owing to incipient decomposition of the fluid, connected with the putrefactive process.

3.—*Of Synovia and of Serous Fluids.*

Recently, in two instances, I have subjected synovia to the air-pump ; in both, it was taken from the human subject—in one, three hours after death—in the other, sixteen and a half. The trials were made, as already described, by introducing the fluid, as it flowed from the joint, through an appropriate opening into distilled water purged of air. In the first example, not a particle of air was disengaged. In the second, two or three very small particles only were separated, giving the idea of adventitious air, entangled during the passage of the fluid from the capsule to the vial ; there was not the slightest general effervescence. At Corfu, also, I subjected synovia to the air-pump, in two different instances—the fatal cases referred to

in the preceding section—and the results were equally negative.

It has been asserted by a distinguished inquirer, that a small quantity of air is not an uncommon occurrence in the synovial capsules: the words of M. Laennec are—"l'on sait qu'il se trouve souvent, dans les capsules synoviales, une petite quantité d'un fluide aériforme."* If so, it might be expected, that air might even more frequently be detected in the synovia itself, on the removal of atmospheric pressure; and that the results I have given above are to be viewed as exceptions. But, with a feeling of the highest respect for the individual quoted, I believe that in this instance, he has received as matter of fact what is not really so, trusting unduly to loose opinion formed on careless observation. For many years, my attention has been specially directed to the subject of air in the cavities of the body, and I can, without hesitation, say, that in no instance, out of several hundred, in which I have examined the joints after death, could I detect in them the presence of any elastic fluid.

Amongst my notes, bearing the date of Malta, June, 1828, I find three cases, in which careful search was made for air in the capsules of the joints and the sheaths of tendons, by an examination of the parts under water,—the bodies having been immersed in a bath for the purpose; one, six hours after death, another eleven, and a third twelve. In

* *Traite de L'Auscultation Médiate*, Paris, 1826, tom. ii. 445.

each instance, the result was perfectly negative. As synovia, from its viscid nature, is peculiarly apt to froth or entangle air, it is not surprising that such an opinion as that referred to by the very eminent French pathologist should have arisen.*

Of serous fluids I have hitherto made but few trials; they have been confined to the liquor pericardii and

* The same opinion is expressed by Laennec in another passage, the correctness of parts of which are very questionable. On account of the importance of the subject, practically as well theoretically considered, I shall quote his words at length: "Tous les bruits qui se passent dans l'intérieur du corps, et que l'on peut entendre à l'oreille nue, sont dus aux mouvemens de quelque substance qui se trouve en contact avec un gaz. C'est ainsi que l'on entend les borborygmes dans les intestins, la fluctuation hippocratique dans le pneumothorax avec épanchement liquide, et même celle qui a lieu dans l'estomac, le bruit de la crépitation déterminé par l'inspiration ou par les battemens du cœur dans quelques emphysèmes des parois thoraciques, la craquement des doigts chez certain sujets dont les articulations contiennent habituellement un gaz, un bruit analogue et accompagné de crépitation manifeste sous la main, dans les pneumarthroses qui succèdent fréquemment au rhumatisme articulaire, et particulièrement dans l'articulation du genou."—Op. cit. ii. 456. In this passage some important truths are mixed with opinions of more or less doubtful accuracy. As the vibrations essential to sound can take place in a fluid, and an elastic fluid is not absolutely required, there is no good reason why several of the sounds mentioned may not be produced without the presence of air; I shall notice only one in particular by way of illustration, that of the cracking of the fingers—"le craquement des doigts:"—this sound was produced in two of the fatal cases noticed in the text, in which the capsules of the joints and sheaths of tendons were examined for air, and without a particle being detected, even in the joints which emitted the sound on extension. And this instance, I apprehend, is applicable by analogy, to all others in which firm or hard surfaces are subject either to attrition or concussion.

the fluid of the lateral ventricles of the brain. In three different instances (the limit of these trials) in which these fluids, a few hours after death, were subjected to the air-pump, no indications were afforded of the disengagement of air, excepting a minute bubble or two, which, probably it is right to infer, arose from air entangled in transferring the fluid from the pericardium and the ventricle into the vessel prepared for it. In the instance of the former, it was scooped out with a gallipot, from which distilled water, purged of air, had, the instant before, been emptied; in that of the latter, it was allowed to run into a vial nearly full of water, purged of air, through an opening in the posterior cornu.

In a case of abdominal dropsy, in which it was necessary to draw off the accumulated fluid by the operation of paracentesis, the transparent serum of sp. grav. 1011 was agitated with common air in the double-mouthed bottle already mentioned, without the disengagement of any gas; which may be considered equivalent to a negative result with the air-pump.

As the serous fluids are commonly alkaline, [that is, contain an alkali not saturated fully with carbonic acid, and capable of absorbing a considerable additional proportion,] the probability is, that in their healthy normal state, they never contain this acid free or uncombined, nor is it likely, reasoning *a priori*, that they contain any other gas in a state of freedom or mere mechanical solution.

In a state of disease, however, and especially of fatal disease, I believe the presence of carbonic acid in these fluids is not an uncommon occurrence. When treating of the blood after death, I have mentioned that I have frequently obtained indications of this gas in serum effused in excess into the great cavities, by agitating a few ounces of it in the double-mouthed bottle, with twice or thrice its volume of common air. In these cases, the blood, it should be remembered, was found by the same test, to abound more or less in the same acid gas. The source of the carbonic acid, in the instances referred to, and whether besides this gas, any other gas is mixed with it, are undetermined points, and seem to be fit subjects for farther and special inquiry.

4.—*Of the Humours of the Eye.*

The internal component parts of the eye being from their nature and situation very suitable for the trials under consideration, I have in three different instances subjected them to the experiment of the air-pump. In each instance, the organ was extracted a few hours after death; a vial having been prepared full of water purged of air, the sclerotic coat was partially divided, and the lens and a portion of the aqueous and of the vitreous humour, was received into the water. On one occasion, a small piece of the retina accompanied the lens, and on another, minute portions of the ciliary processes. On

exhaustion in each example, the result was similar; no air was disengaged. In the last of the three trials on the eye from different bodies, the contents of one eye were examined three hours after death, and of the other eye the following day, twenty hours later: the result was the same in both. The thermometer during the time, in the room where this body was deposited, was about 58° .

I am not aware, that air has ever been observed in the eye during life, or that there is any well-authenticated instance of its having been detected in this organ after death, excepting in cases of very advanced putrefaction.

5.—*Of the Solid Parts of the Body.*

It would be tedious to give in detail an account of the experiments which I have instituted in quest of air in the solid parts of the body. It may suffice to state the general result, or rather the impression made by the results on my mind, which is, that no internal organ, in its healthy normal state, contains air capable of being extricated by the removal of atmospheric pressure by the air-pump, excepting those, [the lungs especially] which are designed to be its recipient.

In no instance, in which I have tried the substance of the brain, before putrefaction had commenced, has any air been disengaged from it *in vacuo*,—a central part having been selected, in which there were no obvious vessels, and carefully cut out with a fine sharp

scalpel just dipped in water purged of air, and as soon as cut out, immersed in the same water.

But, in the instances of the other viscera, I have always found more or less air disengaged, on subjecting portions of them in the same manner to the action of the air-pump. The proportion of air evolved appeared to accord with the degree of vascularity of the part. This, I apprehend, is what might be expected *à priori*, taking into account the rapidity with which air enters the arteries, when they are divided, owing to their expansion on the access of air, depending on their elasticity. I shall offer a partial proof of an experimental kind.

Two rabbits of the same age were killed; one, by dividing the great cervical blood-vessels; the other, by a blow on the occiput. The thorax of each was opened before they were cold. In the heart of the first-mentioned, there was evidently air, and even in the coronary vessels. In the heart of the second, there was no appearance of air; it was moderately distended with blood, and its vessels were full of blood. A ligature was applied to the great vessels of each, and the hearts were removed and put into water. The first swam; the second sank. Under an exhausted receiver, the first was amazingly distended; its blood-vessels also were distended, and their very minute ramifications were beautifully displayed, variously anastomosing: very little of the air was disengaged. The second was not at all distended; a few bubbles of air only were given off,

and these came principally from the ends of the divided vessels; the coronary vessels did not exhibit the slightest change, and not a particle of air could be discovered in them.

These experiments were made at Corfu. Recently, I have instituted similar ones (using an air-pump of the best construction) on the carotid artery, containing blood secured by a ligature, and with the like result, as has already been described, when treating of the blood, in connexion with respiration; and demonstrating, that even if the blood contain carbonic acid gas capable of being extricated by the air-pump, this extrication cannot take place, so long as the fluid is contained in the vessels, the pressure of their sides being sufficient, it would appear, to retain the elastic fluid, when present.

Prochaska, in his ingenious treatise on the organism of the human body and its vital processes, whilst he denies, as has been already mentioned, that air is to be found in the arteries after death, yet endeavours to shew, that during life, they and other parts of the body partially owe their distension to vapour which, as he supposes, they contain. He says: “*si statum sanguinis in homine vivo et mortuo invicem comparamus, ingens voluminis differentia in utroque adparet; nam qui in vivo homine omnia vasa a maximis ad minima usque repleverat, rubore vitali cutim universam perfuderat, post mortem ex minimis vasis evanescit, cui pallor cutis cadaverosus succedit, venæ subcutaneæ collabuntur, et arteriæ a minimis*

ad maximas usque fere inanes ac vacuæ reperiuntur. Cl. Rosa arteriam vivi animalis sanguine repletam in duobus locis ligaverat, quæ excissa et refrigerata volumen suum ad $\frac{3}{9}$ diminuit, et sanguis hujus contractæ arteriæ filum referens tantum $\frac{1}{3}$ diametri partem explevit; ex quo sequitur, quod sanguinis volumen in arteria viva ad volumen sanguinis arteriæ mortuæ se ut 9 ad 1 habeat.”*

The vapour which Prochaska alludes to, it is hardly necessary to observe, can only have effect, and indeed can exist only, where there is free space, as in an artery not full of blood,—in the pleura or peritonæum—not at every point in contact with their contents. If such a vapour as he supposes exist in the body, and if the results of Rosa’s experiments quoted by him, are owing to its action, it might be expected to be of a peculiar kind, not merely aqueous vapour, but of some other fluid capable of becoming elastic, and of exerting a distending force at the ordinary temperature of the blood. I shall describe what I have witnessed in repeating the experiments, made with all the care I could use, to endeavour to satisfy myself on a point of very great interest, at least, thus hypothetically viewed.

1st Experiment.

Laid bare and fully exposed the carotid artery of

* Disquisitio Anat. Phys. Organismi Corp. human. ejusque Processus Vit. Auctore G. Prochaska. Vien. 1812, p. 86.

a sheep, on the 10th May, when the temperature of the air was about 60° ; the 8th nerve was separated from the artery. Applied loosely two ligatures to the exposed part; the superior was first drawn tight; there was no apparent increase of the volume of the vessel below. Immediately after tying tightly the lower one, the included space was measured by a compass and found to be exactly 1.50 inch, and the diameter of the included vessel .20 inch. The part was now cut out; the artery was first divided above the superior ligature; blood flowed moderately as from a vein from the open extremity; it was next divided below, when the blood gushed out with violence. The portion removed was again immediately measured; its diameter was not perceptibly changed; but, in length from 1.50 inch, it was contracted to 1.20. It was instantly placed in water purged of air by the air-pump; and about an hour after, placed under a receiver for exhaustion. On exhausting the receiver, I narrowly watched the artery; but could not perceive the slightest distension of it, such as might be expected if it contained any fluid more volatile than water; nor did any take place when the vacuum was as complete as it could be made. Measured again, now it was quite cold, and after having been taken out of the water, its diameter appeared unaltered, but its length a little increased, namely to 1.30 inch.

2d Experiment.

This experiment was conducted precisely in the same manner as the preceding. The portion of carotid artery included between the two ligatures, measured before excision, was 1·40 inch in length, and ·20 in breadth. Immediately after excision the length was 1·32, the breadth remaining unaltered. It was subjected to the air-pump in the same manner and after the same time, and with the same negative result. Now on admeasurement, no change could be detected in its dimensions, either in length or diameter.

In both instances, the artery which was cut out was unusually tense and hard; and when punctured, the contained, and as it would appear, compressed blood sprang forth with force, and to the distance of about two feet, as if from an animal alive.

These results, carefully considered, I apprehend, can in no wise be received as favourable to Prochaska's views of the existence and operation of an expansive vapour in the living body. As the contraction of the artery took place immediately on its division and excision, agreeably to what is known of the properties of arteries, and was not increased by cooling, it is not for, but in opposition to, the argument used by the learned professor. The phenomena themselves which he endeavours to explain by means of his imagined cause, do not require it. The heart's action during life, seems sufficient to

account for the expansion of the vessels, even to the capillaries: and its cessation in death, conjoined with atmospheric pressure, and the elasticity of the arteries and the force of gravitation, to explain the emptying of the arteries and superficial vessels, and the consequent pallor, and shrinking of the surface, which is then witnessed; and as is also witnessed in a less degree, in momentary syncope, the same cause, as regards the heart, taking effect,—although unaccompanied by reduction of temperature, such as would be required to affect the condition of vapour, were it in existence.

Prochaska makes many excellent remarks on the porosity and permeability of vessels, admitting of a penetration by moisture, which amongst parts in contact, will act the same office as vapour; indeed, in stating his views on the subject of the porosity of the vessels,—he has adduced some curious facts, on an extension of which recently, the doctrine has been grounded of endosmosis and exosmosis.

Before coming to a conclusion, it may be right to notice an objection which may be made to the results of my experiments in search of air in the animal fluids and solids, and the inference drawn from them, that no air in a state capable of being removed by exhaustion exists in them. It may be said, that animal membranes are permeable to air, and that therefore they must contain it, and not they alone, but also the fluids within them. This is true of the dead membranes,—especially the thinnest of

them, and after a time. But it remains to be proved, that it is true of membranes generally, and especially during life: and until proved,—it ought not be admitted. But even were proof of it afforded, it does not necessarily follow that the pervading air should be found unaltered within: the oxygen, it is probable, would combine in a peculiar way to the colouring matter of the blood; and the azote might also immediately enter into combination. The effect of porous spongy platinum in effecting the combination of certain gasses is as well known as it is extraordinary: other matters seem to have an analogous effect: and there is no reason why porous living parts should not act in the same way.

VII.

ON THE AGE OF MORBID ADHESIONS, AND OF THE FLUID FOUND IN THE PERICARDIUM AFTER DEATH.

It is an opinion pretty generally prevalent, that the age of these morbid adhesions,* which are so frequently met with in the dissection of bodies, connecting together serous membranes, may be guessed at by their comparative degree of strength; thus, weak adhesions are usually considered of recent origin,—and firm adhesions as being of long standing. Is this opinion correct; and does it agree or not with the properties of coagulable lymph, of which these adhesions are principally formed?

Many circumstances, of which it will be sufficient to mention a few, appear to me, decidedly in favour of the negative conclusion.

Wounds, it is well known, which heal by the *first intention*, are often firmly united in twenty-four hours.

In the same space of time, when inflammation has

* I use the term in its common professional sense, signifying the matter constituting the morbid connexion of parts.

been artificially excited, I have witnessed the formation of strong adhesions. An instance may be given. At Colombo, in the island of Ceylon, in January, 1819, I made the following experiment on a young dog, nearly full-grown. By an opening made with a scalpel, between the ribs of the right side of the chest, about a scruple of arrack was injected into the cavity of the pleura. The lung was slightly wounded; air passed freely through the opening, and a little frothy mucus was discharged. The animal seemed at first to suffer pain and to be languid; but left to himself, he gradually recovered, and in the course of the day took some food. At the expiration of twenty-four hours, the dog was hot, but apparently not suffering pain: he was running about and the wound was closed. The dog was now killed and was almost immediately examined. A good deal of coagulable lymph, which had the appearance of being vascular, was found under the skin round the wound,—connecting the cutis and the intercostal muscles. The adjoining cellular membrane was inflamed. Some bloody serum was effused into the right cavity of the chest. Many pretty firm and long adhesions had formed between the pleura pulmonalis and costalis,—each of which was of a reddish hue. There were also many adhesions between the pleura and pericardium, and the pericardium itself was inflamed,—its surfaces adherent, and, consequently, its cavity obliterated.

The coagulated lymph of the buffy coat of the

blood may be used as an illustration of the short time in which strong adhesions may form; and I do not know a better confirmation of the fact. *Liquid*, when the blood is drawn, coagulable lymph gradually becomes—*first, viscid*, and afterwards solid. In the viscid state, as I have frequently observed, when it is still transparent, it has the tenacity of mucus, and admits of being drawn out into fibres and bands, which soon becoming firm and opaque very well represent the ordinary adhesions of the lungs,—and in a very few hours attain their maximum of strength.

This viscosity, which coagulable lymph acquires, in passing from a liquid to a solid form, has not, that I am aware, been noticed by authors; and the formation of adhesions is usually explained without reference to this quality.*

1. It is believed by many writers and by some of high authority, that the small portions of serous fluid, which are found after death, in the cavities of serous membranes, especially in the pericardium and the ventricles of the brain, may have been poured out after the cessation of life.† I am not aware that this

* Vide "The Morbid Anatomy of some of the most important parts of the Human Body," by Matthew Baillie, M.D., F.R.S., fifth edition, p. 6. This reference was made in 1822, in a paper published in the Philosophical Transactions for that year; since that time I do not recollect any writer who has insisted on the above mentioned important property of lymph.

† Sauvage's Nosologia Method. *Ephialtes ex Hydrocephalo*. Cours d'Anatomie Medicale, par Antoine Portal, tom. iv. p. 54, 8vo., Paris, 1803.

opinion is supported by any precise facts. As a theoretical conclusion its correctness seems doubtful. I have endeavoured to put it to the test of experiment, and the result has been unfavourable. I have notes of three different experiments on dogs, which were made in Ceylon in 1818,—all of which seem to show that, under ordinary circumstances, no effusion of serum or exudation, so as to occasion accumulation, takes place after death. The experiments were briefly the following. In each instance a healthy dog was suddenly killed by a blow on the occiput; the cavity of the chest was instantly laid open and the pericardium inspected; a small quantity of serum found in it, was removed by a sponge, and the incisions were then closed by sutures. At the end of twenty-four hours the sutures were divided, and the pericardium was again examined. In no instance had a single drop of fluid collected in the pericardium; and yet, in two instances, the right auricle and ventricle were considerably distended with blood.

If these results be conclusive against fluid being effused into the pericardium, after death, in dogs, the inference from them admits of being extended, by analogy, to other cavities of the same texture and to man; and I am not acquainted with any pathological observations in opposition to it. The discovery of serous effusions in examinations *post mortem*, no symptoms of their occurrence or existence having been noticed during life, is surely no evidence of

their having taken place after the cessation of vital action. It is too well known to be insisted on, that large portions of fluid may accumulate in the pericardium, and even in the ventricles of the brain, without a single symptom indicative of the occurrence.

These remarks were written in 1822, and were published in the Philosophical Transactions for that year. Referring to an able work on Physiology, now in progress of publication by Professor Burdach, I perceive that this ingenious inquirer and learned author objects to the results of my experiments, on account of the pericardium having been exposed to the pressure of the atmosphere, which, he is of opinion, would arrest the exudation, "*le pericarde ayant été ouvert et exposé à la pression de l'atmosphère, la sécrétion ne pouvait plus s'y accomplir.*"* This objection appears to me rather specious than well founded. It is true that the air is excluded from closed sacs, such as the pleuræ and pericardium; but I do not believe that the pressure of the atmosphere is prevented, inasmuch as atmospheric air can enter through the aspera arteria into the lungs, and through the medium of them, act on the pericardium or pleuræ,—yielding membranes, and ordinarily in contact with the organs which they contain,—and consequently not offering any true

* *Traité de Physiologie considérée comme Science d'Observation*, par C. F. Burdach, Professeur à l'Université de Königsberg. Traduit de l'Allemande, par. A. T. L. Jourdan, tom. viii. p. 509.

vacuum or an approach to it, favourable to exhalation.

Reasoning on the subject and considering the relative qualities of the parts concerned,—the heart, the pericardium, the pleuræ, and lungs,—it appeared to me more probable, that the pericardium would rather lose fluid than gain it after death by exudation, inasmuch as the external layer,—that dividing it from the pleura,—is a thinner boundary, and it may be supposed, more easily penetrated than the internal, comprehending with it the muscular parietes of the heart and its inner lining.

To endeavour to illustrate this view by experiments, I have had recourse to an aqueous solution of the triple prussiate of potash* (ferro-cyanuret of potassium).

* The solution of this salt is peculiarly well fitted for the above purpose, both on account of the facility with which its presence may be detected in animal fluids, and its not apparently having any astringent or other effect on the delicate textures of the body. The experiments of Hering on the injection of this salt into the veins of horses may be adduced in proof and illustration; as may also some experiments which Dr. Wollaston made on the taking of the salt internally in the year 1798, and some others made by myself in 1823. From Professor Burdach's account of Hering's experiments given at page 344 of the 8th vol. of his Physiology, it appears that the prussiate was often detected in the substance of the kidneys, at the expiration of a minute after its injection into the vein; and that it ceased to be detected in the blood in from fifteen minutes to five hours,—although it continued appreciable in the urine very many hours longer, even two days.

Dr. Wollaston, in his paper "On the non-existence of Sugar in the Blood of Persons labouring under Diabetes Mellitus," published

Experiment 1.

Two hours after death from pulmonary consumption, the jugular vein of a body was opened,—six

in the Philosophical Transactions for 1811, has noticed the results of some of his experiments, when engaged in some similar trials. He favoured me with a copy of his original notes, which, as they may interest the physiological reader, I shall introduce verbatim. His not being able to detect the salt in the blood, is explained by the results mentioned as obtained by Hering. It may be premised that the figures in the first column indicate the time when the salt was taken and the urine was tested.

December 15th, 1798.

vii.		pruss. gtt. xxv.=gr. iii.	} 2.5
vii.	15,	not blue.	
viii.	5,	rather blue.	
	25,	blue.	
	45,	ditto.	
x.	10,	blue.	
xi.	20,	blue.	

December 17.

ii.	30,	pruss. gtt. xxx.	} 1.20 shortest time of observed blueness.
iii.	20,	not blue.	
iii.	50,	blue.	
iv.	20,	ditto.	
vi.	30,	ditto, strong.	

December 26.

vi.	50,	pruss. 3ß	} 2.40
viii.		ditto.	
viii.	45,	ditto.	
ix.	30,	rather blue.	
x.	30,	rather blue.	
	50,	V. S. not blue, i. e. the serum.	

In my experiments a much larger quantity of the salt was taken, viz. 40 grs. of the crystallized salt dissolved in an ounce and half of water,—and as might be expected, the results were more decided. After 15 minutes it was first detected in the urine, and it continued

ounces of blood were allowed to escape, and then twelve ounces of the solution* were injected in the direction of the heart, and ligatures immediately applied to the vessel. The *post-mortem* examination was made on the following day, the 23d September, twenty-three hours after death.

to be detected for the space of 33 hours. The following is a copy of the notes made at the time:—

June 12th, 1833.

- xii. 40, took the salt.
- 42, no effect on urine.
- 48, ditto.
- 55, very slight bluish tinge.
- 1.5 slight blue.
- 15, blue.
- v. 0, ditto.
- vii. 0, ditto.
- x. 30, slight blue.

June 13.

- viii. slight blue.
- i. P.M. ditto.
- ii. faint bluish tinge.
- x. very faint.

June 14.

- viii. A.M. no perceptible bluish tinge.

I find it remarked, that almost all the urine voided during the 33 hours that the salt was appreciable, was tested,—and that the quantity of iron thrown down was very small, conveying the idea that only a small portion of the triple-prussiate reached the bladder,—and giving rise to the conjecture that the greater part of it was decomposed in the system.

I may add by way of precaution,—that the day after taking the very large dose of the triple prussiate, I felt unwell,—experiencing slight vertigo and tendency to syncope.

* 3 grs. of the salt were dissolved in 12 ounces of water.

Not quite half an ounce of fluid was found in the pericardium. On the addition of a little solution of green sulphate of iron, no immediate change of colour was produced; on admixture of a little nitric acid, coagulation was occasioned, and a faint but distinct greenish hue appeared.*

There was some crassamentum and cruor, and fibrinous concretion nearly colourless in the right auricle and ventricle, — the fibrinous concretions chiefly in the latter. A portion of this fibrin, moistened with the solution of sulphate of iron and a little nitric acid, became blue. In the left ventricle there was a little fibrinous concretion: this tested in the same way, acquired a greenish hue which was only just perceptible. Portions of the lungs were similarly tried. Some portions became greenish; some blue; the blue colour appeared chiefly in the course of the blood-vessels: even the walls of a large tubercular excavation, covered with false membrane, acquired a just perceptible greenish hue.

These results seem to indicate that there was a slight exudation from the heart into the pericardium, but not exclusively so, nor to the extent to occasion the accumulation, which is corroborated by other circumstances.

The serum from the blood, which flowed from the jugular vein after death, (it coagulated and afforded

* Serum prevents the change of colour, — the instant coagulation is produced by nitric acid, the colour appears.

clear serum and well formed crassamentum) was of a bright yellow hue; the liquor pericardii was nearly colourless.

Three drops of the solution of triple prussiate mixed with half an ounce of serum, tested in the same manner as the liquor pericardii, exhibited as nearly as possible the same degree of colour.

Whence it may be inferred that extremely little of the solution passed into or was retained in the pericardium; that the greater part of it passed into the lungs,—and that the liquor pericardii was not a *post-mortem* exudation, but a vital secretion.

Experiment 2.

On the 15th September, injected into the jugular of a body, extremely emaciated, which had been dead nineteen hours, nine ounces of a stronger solution of the triple prussiate;* and made the *post-mortem* examination two hours after. The circumstances of the case were favourable; there was no serious disease of the lungs, none of the heart; the age of the deceased was 37; the principal lesions were softening of the brain, especially of the fornix, and dropsical effusion into the cavity of the abdomen.

In the pericardium about an ounce of fluid was found: tested by the salt of iron and nitric acid, it indicated the presence of a very minute quantity of the triple prussiate; it acquired a very light greenish hue.

* 6 grs. of the salt were dissolved in 12 ounces of water.

There were about three ounces of a similar fluid in the right pleura, and the effect of the test on it was similar : the greenish hue imparted was perhaps a shade lighter. There was very little cruor, crassamentum, or fibrinous concretion in any of the cavities of the heart, and no appearance of any of the solution. A semilunar valve of the pulmonary artery tested was strongly coloured bluish-green ; the substance of the lungs exhibited on trial the same hue in a less degree, and it appeared even faintly in the frothy fluid contained in the bronchia.

May not these results be considered a confirmation of the preceding, so far at least as regards the wide spread of the solution,—and there being no proof of any accumulation of it in the pericardium. It may be said, that accumulation in the pericardium could hardly be expected, as the greater part of it seems to have passed into the lungs. But is it not probable, that when serum separates from the blood in the heart after death, it will pass out through that channel, chiefly, if there is any undue pressure, and that the exudation into the pericardium, will be trifling and even less than from the pericardium into the pleura.

Experiment 3.

On the 18th September, twelve hours after death from complicated organic disease of the lungs, introduced about four ounces of a weak solution of the triple prussiate into the left pleura, through an

opening made by removing a portion of the false ribs; the deceased was 26 years of age. The usual inspection was made three hours and half after. The pericardium was carefully laid open; it contained about half an ounce of fluid, and by the test before mentioned, it was proved, that a very little of the solution had penetrated into the pericardium and was mixed with it; it acquired a distinct greenish hue. There was a very little fluid in the right pleura; and this on trial afforded proof, by the change of colour, of the saline solution having reached so far.

Experiment 4.

On the 19th September, an hour and half after death, the consequence of articular suppuration, after an accident, (the deceased was 45 years of age,) opened carefully into the pericardium, avoiding the pleuræ; and introduced about four ounces of the solution. The opening was closed by suture of the integuments. The *post mortem* examination was made 21 hours after. The left lung contiguous to the pericardium was moist; a few drops only could be collected from its surface. This minute quantity, on the addition of a drop of the ferruginous solution, became dark-green, and on the addition of a drop of nitric acid, dark blue. The blood in the heart was too thick to admit of being tested.

All these results incontestibly prove a permeability of the membranes after death, admitting of the exu-

dation of fluids. They also seem to shew that the amount of exudation is inconsiderable ; and to render it highly improbable, that in the instance of the pericardium, pleuræ, or ventricles of the brain, or indeed in the instance of any closed cavity of the body, accumulation, (the consequence of exudation,) should take place.

Other facts might be brought forward in support and illustration of the preceding views. I shall very briefly mention a small number.

1. There is no relation between the quantity of blood in the cavities of the heart and the quantity of fluid found in the pericardium. Of this fact, I have satisfied myself by very many observations, taking into account the time elapsing between death and the examination of the body. Occasionally, the one is large and the other small, and *vice versâ*.

2. There is no relation, as far as my experience extends, between the quality of the fluid in the pericardium, and the serum of the blood. I have mentioned an instance in which one was colourless, and the other bright yellow. I have witnessed many instances in which the serum of the blood in the heart and great vessels was coloured red, some of the colouring matter of the red particles having been dissolved by it, and in which the same colour was exhibited in the inner lining membrane of the heart and great vessels, the effect of staining, without the liquor pericardii being tinged.

3. Coagulable lymph is very rarely found in the

liquor pericardii. But, were the fluid a *post mortem* exudation, it might be expected, that coagulable lymph would be very frequently observed in it, inasmuch as in the majority of instances the blood is slow in coagulating; a portion of it is commonly found liquid in the heart very many hours after death, and which coagulates on exposure.

4. The gall-bladder distended with bile, the urinary-bladder with urine, in fatal cases of apoplexy or palsy, the synovial membranes with synovia, part with their contents with extreme slowness. In no instance have I ever met with synovia in the cellular tissue adjoining the capsule of a joint abounding in synovia. I am not aware of urine having ever been found in appreciable quantity in the cavity of the pelvis after death with distension of bladder.* How small is the exudation from the gall-bladder, and how slow in taking place. Were it not for its colouring quality, the exudation would not be appreciable.

If these views be correct, the question of permeability of membranes and exudation of fluids in consequence, need not greatly embarrass the patholo-

* On the 11th October, 16 ounces of a weak solution of triple prussiate of potash (strong enough, however, to become dark-blue when mixed with a salt of iron) were injected into the urinary-bladder of a young man two hours after death from pulmonary consumption. The *post mortem* examination was made 24 hours after; the cavity of the abdomen was first opened into and carefully examined; the only fluid found was in the pelvis, between the bladder and rectum, not quite half an ounce, and, tested by iron, no trace of the prussiate could be detected in it; it appeared to be a weak serous fluid,—it was rendered turbid by nitric acid.

gical inquirer. I apprehend, that if no colouring matter of the blood is found mixed with a fluid in a closed serous cavity, it may fairly be inferred, that it was secreted during life; that if the colouring matter of the blood be found intermixed, especially in solution, the inference must be drawn that an exudation has taken place, the amount of which may perhaps be measured with tolerable accuracy by the quantity of colouring matter.

The solution in the serum of the blood of the colouring matter belonging to the red particles is connected with putrid decomposition, and is one of the first indications of it. The permeability of membranes, if not another effect of the same change, is, I am satisfied, greatly increased by it. This is very manifest from the inspection of bodies at different intervals after death, and at different seasons of the year: I believe it may be laid down as a rule that the circumstances favourable to putrefaction, are equally so, to permeability and exudation.*

* The difference of effect is well shewn in the instance of the urinary bladder. On the 27th of September, when the weather was cool, water, urine, and cruor of blood, were respectively put into sheeps' bladders, quite fresh, and suspended in jars, covered from the air. For three hours not a drop of either fluid had passed through; after twenty-four hours, a small quantity of each was found in the receivers, and the serum was coloured red, and even contained red particles. Through a bladder, allowed to become putrid, water passes as through coarse filtering paper. The same effect of putridity, in increasing permeability, is strongly marked in the leather made in warm and in cold climates. In the former, where it is difficult to arrest the putrefactive process, the leather is in a high degree

Professor Burdach, when treating on what he considers "secretion after death," adduces the results of the experiments of Ségalas, who, in the examination of animals, found, that if it was delayed to twenty or thirty hours after the death of the animals, the blood was thicker in the right side of the heart, and diminished in quantity by the delay, and that sanguinolent serum had collected in the serous cavities, especially in the pleura.*

These results, it appears to me, may be considered analogous to those described in the first experiment which I have related. As in that experiment the greater part of the fluid seems to have passed into the lungs through the natural channel of the pulmonary artery, — so in those of Ségalas, the cruor might have had issue in the same way, and if putrefaction had commenced, as seems indicated by the coloured serum, an exudation might have taken place into the serous cavities, and especially from the lungs into the pleuræ.

porous, well adapted for use in a high temperature; in the latter, not having suffered from putrefaction, it is dense and almost impervious. It is equally well fitted for use, as a protection from cold and moisture.

* Op. cit. vol. viii. p. 509.

VIII.

ON THE SPECIFIC GRAVITY OF DIFFERENT PARTS OF
THE HUMAN BODY.

ON this subject I was induced to make a set of experiments in 1827, from the belief, that on account of its elementary bearings and probable applications, it was deserving of more extended investigation than it had then received.

Most of the experiments were made in our military hospitals at Corfu, where advantage for carrying on the inquiry was taken of the *post-mortem* examination of the bodies of deceased soldiers of our own regiments, who died under medical treatment, and whose cases, according to the usage of the service, were recorded.

The parts submitted to trial, were weighed almost immediately after they were taken from the body, in every instance in less than six hours, and generally within two. When dissection was necessary, they were carefully dissected out; they were freed as much as possible from contiguous and adhering textures, and especially from fat; and without being

previously washed, they were deprived of superfluous adhering fluid, by being gently pressed between folds of blotting paper.

For the sake of brevity I shall give the results in a Tabular form: in expressing them, distilled water, the usual standard of comparison, is considered as 1000. The Roman numerals in the second column, indicate the bodies to which the parts subjected to experiment, belonged.

Table of Specific Gravities of different parts of the Human body.

No.	Subject.	Part tried.	Result.
1	xi.	Nail of thumb, dissected off entire	1197
2	ii.	Cuticle of sole of foot	1190
3	—	Do. with adhering cutis	1180
4	xi.	Skin of back of thumb, free from fat, with cuticle undetached	1100
5	iii.	Skin of chest, over the pectoral muscle	1108
6	—	Do. with a thin layer of adhering fat	1050
7	iv.	Skin of epigastric portion of abdomen	1103
8	—	A front tooth of a young man, not decayed, extracted because loose, owing to disease of the gum	2240
9	—	Root of the same, the air which was in its cavity expelled	1950
10	—	Crown of the same	2380
11	—	First molar tooth of a man, about forty years old, extracted on account of pain, connected with a slight caries	2142
12	—	Roots of the same	2113
13	—	The whole of the upper part of the same, above the gum	2313
14	—	The top of the crown of the same, consisting chiefly of enamel	2620
15	—	A molar tooth not complete, the roots not formed, from the skull of a young woman of Ipsara, which had been exposed about two years to the open air	2500
16	—	A portion of parietal bone from the same skull	1944
17	ix.	A portion of pars petrosa of temporal bone	1852

No.	Subject.	Part tried.	Result.
18	iv.	Parietal bone freed from pericranium .	1772
19	—	Eighth rib freed from periosteum .	1383
20	iv.	Fat from beneath the integuments of abdomen*	0942
21	—	Dura mater	1090
22	v.	Tunica albuginea testis	1088
23	xii.	Sclerotic coat of eye	1091
24	—	Cornea	1076
25	—	Lens	1100
26	v.	Ligamentous sheath of corpora cavernosa penis	1097
27	ix.	Aponeurosis covering vastus externus muscle .	1080
28	vi.	Anterior portion of last lumbar intervertebral substance	1092
29	xii.	Anterior and outer portion of intervertebral substance, from the lumbar part of spine .	1104
30	—	Upper portion of the same, containing some of the articulating cartilage . .	1110
31	—	The portion beneath, free from articulating cartilage	1098
32	—	Central parts of the same, soft and pulpy .	1062
33	x.	Tendo Achillis	1080
34	vi.	Ligament of patella	1104
35	—	Cartilage of knee-joint	1073
36	iii.	Pericardium	1054
37	v.	Cellular part of corpora cavernosa penis .	1086
38	iv.	Thoracic aorta	1086
39	vi.	Arch of aorta	1080
40	—	Upper part of femoral artery	1071
41	—	Middle part of the same	1061
42	—	Upper part of the internal iliac artery .	1060
43	vii.	Half of a portion of aorta, just below its arch, divided longitudinally	1086
44	—	Other half of the same, its outer fibrous coat detached	1077
45	—	Lower part of aorta, just before its termination	1074
46	vii.	Internal iliac artery	1068
47	v.	Abdominal vena cava	1061
48	xiii.	Vena cava, close to the emulgent vein .	1061
49	—	Emulgent vein	1061
50	—	Upper part of femoral vein	1066
51	—	Contiguous vena saphena major	1071
52	vi.	Two of the largest nerves of the femoral fasciculus in the groin	1111
53	iii.	A portion of cerebrum (medullary and cortical substance intermixed)	1040

* Its specific gravity was ascertained, indirectly, by means of that of a mixture of alcohol and water, in which, when submersed, it remained stationary.

No.	Subject.	Part tried.	Result.
54	—	A portion of cerebellum	1043
55	iv.	Pons varolii	1033
56	—	Medulla oblongata	1037
57	i.	Upper part of spinal cord	1035
58	v.	Substance of testicle	1041
59	iii.	Substance of liver	1053
60	iv.	Ditto	1035
61	i.	Ditto	1069
62	v.	Ditto, (Lobulus Spigelii)	1066
63	vi.	Ditto	1050
64	vii.	Ditto, (Lobulus Spigelii)	1059
65	viii.	Ditto	1042
66	ix.	Ditto	1045
67	iii.	Substance of spleen	1044
68	iv.	Ditto	1064
69	v.	Ditto	1070
70	vi.	Ditto	1048
71	vii.	Ditto	1058
72	viii.	Ditto	1061
73	ix.	Ditto	1060
74	iii.	Substance of pancreas	1047
75	v.	Substance of kidney	1050
76	vi.	Ditto	1050
77	viii.	Supra-renal gland	1048
78	viii.	Thyroid gland	1060
79	v.	Concretion of fibrin from ventricle of heart	1057
80	xiv.	Substance of lung	1054
81	iii.	Hepatized lung	1043
82	vi.	Enlarged lymphatic gland, contiguous to the lower part of abdominal aorta	1046
83	iv.	Muscular substance of left ventricle of heart	1049
84	—	Great pectoral muscle	1058
85	vii.	Sartorius muscle	1056
86	xi.	Lower part of œsophagus	1040
87	—	Cardiac portion of stomach	1048
88	—	Pyloric portion	1052
89	—	Upper part of duodenum	1047
90	—	Upper part of jejunum	1042
91	—	Lower part of ileum	1041
92	iv.	Middle part of ileum	1044
93	xi.	Sigmoid flexure of colon	1042
94	—	Hair	1280
95	—	Ditto	1293
96	—	Ditto	1278
97	—	Ditto	1290
98	—	Ditto	1275
99	—	Ditto	1345
100	—	Ditto	1323
101	—	Ditto	1300

The fatal cases to which the Roman numerals refer were the following.

i.—Aged twenty-seven years; was admitted into hospital on 29th August; and died 11th September of peritonitis which followed perforation of the ileum, the effect of chronic ulceration. The liver and upper part of spinal cord appeared to be quite sound. The dissection was commenced eight hours after death.

ii.—Aged thirty-nine years; was admitted into hospital 29th October; died 31st October, of remittent fever, complicated with bronchitis. Dissection eighteen hours after death.

iii.—Aged twenty-eight years; was admitted into hospital 16th July; died 9th of September, of pneumonia. There was much fluid in the ventricles, and between the membranes of the brain; the substance of the brain appeared to be sound. The lungs were variously diseased; hepatized in part, in part dropsical, and tubercular. The liver exhibited slightly the nutmeg-like section. The spleen was not enlarged, but rather of a brighter red and harder than natural. Dissection seven hours after death.

iv.—Aged thirty-four years; was admitted into hospital 9th of September; died 10th September, of (as it was supposed) remittent fever. Blood was found extravasated on the surface of the cerebrum: the trachea and bronchia were redder than natural;

the œsophagus was dark red. The liver was of the colour nearly of yellow wax; the spleen firmer than natural. Dissection sixteen hours after death.

v.—Aged twenty-six years; was admitted into hospital 20th September; died 27th September, of (as it was supposed) remittent fever. The trachea and bronchia were very red; the œsophagus was highly inflamed. The bile in the gall-bladder was black and offensive to the smell; the gall-bladder itself was in a state of incipient gangrene. The common gall-duct was pervious; the liver and spleen were apparently sound. Dissection nineteen hours after death.

vi.—Aged twenty-two years; was in hospital 116 days; died 2d October, of chronic dysentery, complicated with tubercular phthisis, — that is, with tubercles, and vomicæ or cavities in the lungs. The liver was very voluminous, and rather paler and firmer than natural. The spleen was large and firm. Dissection nine hours after death.

vii.—Aged twenty years; was admitted into hospital 30th September; died 5th October, of remittent fever. No organ but the spleen was particularly diseased. The spleen was of great bulk, of very soft consistence, like the stale crassamentum of blood; and after three or four hours' exposure to the air, it became putrid. From some experiments which I made on it, it appeared to contain a large proportion of coagulable lymph effused into it, and a very small proportion of the red particles of the

blood.* This state of spleen, I may remark, almost always presents itself in the fatal cases of remittent fever in the Ionian Islands. The liver was voluminous, soft, and of a brownish grey colour. Dissection twelve hours after death.

viii.—Aged twenty-five years; was admitted into hospital 18th November; died 23d November, of (as it was supposed) remittent fever. The lungs, bronchia, trachea, œsophagus, and colon, were more or less inflamed; the mucous coat of the colon was in places gangrenous. The liver was rather softer than natural; the spleen was apparently sound. Dissection twenty-four hours after death.

ix.—Aged forty-one years; was admitted into hospital 26th November; died 27th November, of inflammation of the bronchia and trachea, supervening on latent tubercular phthisis. The spleen was of natural appearance; the liver exhibited the nutmeg-like section. Dissection eighteen hours after death.

x.—Aged twenty-eight years; was admitted into hospital 29th September; died 22d October, of chronic dysentery, after remittent fever. Dissection nine hours after death.

xi.—Aged thirty-nine years; was admitted into hospital 5th September; died 13th November, of

* It probably also contained pus as well as coagulable lymph; in softened spleens connected with other diseases, I have recently found distinct pus-globules, on submitting the poultaceous matter diluted with water to the microscope.

latent tubercular phthisis, and other organic disease, after an attack of remittent fever. The œsophagus was redder than natural, and its epithelium in great part abraded. The jejunum and ileum were rather redder than natural, and the inferior part of the latter was slightly ulcerated. The large intestines generally, with the exception of the sigmoid flexure of the colon, were severely ulcerated. The spleen was rather smaller and firmer than natural; the liver presented the nutmeg-like section. Dissection seven hours after death.

xii.—Aged twenty-three years; was admitted into hospital 29th August; died 8th December, of tubercular phthisis. The body was extremely emaciated; the lens was unusually soft, and the intervertebral substance perhaps softer than natural. Dissection twenty-nine hours after death.

xiii.—Aged twenty-seven years; was admitted into hospital 11th December, died 21st December, of peritonitis, the consequence of perforation of the ileum, the effect of ulceration. Dissection thirty-one hours after death.

xiv.—Aged twenty-nine years; admitted into hospital 18th January; died 7th February, of inflammation of both pleuræ. A collection of pus was found in the anterior mediastinum; about three-fourths of a gallon of serum in the left pleura, and about one-fourth of a gallon in the right. A good deal of coagulable lymph was effused in each cavity, both loose and adhering. The lungs were very little

diseased; the inferior lobe of the left lung which afforded the specimen, No. 80, the specific gravity of which has been given, was compressed by the effused fluid in a very remarkable manner, and was entirely destitute of air. It bore no marks of inflammation or of hepatization; and in sulphurous acid, it expanded like the substance of the corpus cavernosum penis.*

To these brief notices of fatal cases, it may not be amiss to add some particulars relative to the different specimens of hair of which the specific gravities are given. The first three specimens of hair belonged to English women of the same family, between the ages of thirty and forty years; two were light brown; one was dark brown; all three were fine. The hair, No. 96, belonged to a woman of Corfu, aged sixty-six years; it was grey, and very fine. No. 97 belonged to a man of the same island, aged seventy-seven years; it was white, and very fine and soft. No. 98 belonged to an Ipsariot; it was bleached, and probably, in consequence of the destruction of its oleaginous part, its specific gravity

* Even when immersed in distilled water deprived of air, and placed under the exhausted receiver of the air-pump, it gave off only a few adhering air-bubbles. It would be difficult to explain this by the mere pressure of the effused fluid. Such a complete absence of air was probably owing to pressure and absorption acting together; and, in confirmation of this opinion, I may mention, that I have found only a very small quantity of air in the lungs of animals which have been killed by strangulation, by the ligature of the trachea.

was so high.* The hair, No. 99, belonged to a Hottentot woman, a native of Cape Town, about twenty years old; it was black, coarse, woolly, and almost felted together, admirably adapted for protection against the sun; but it had not been exposed to its action.† No. 100 belonged to a young woman of Pitcairn's Island, in the Pacific Ocean, a daughter of Adam, one of the mutineers of the *Bounty*; it was reddish-brown, partially discoloured by the action of the sun's rays; rather coarse, but not in the slightest degree woolly.

On the results generally I shall offer a few remarks:

* This hair was taken from the body of an Ipsariot, which, when I visited Ipsara, in the winter of 1826, was lying exposed with many others, in the battery, called Fatellio, where they had fallen, making a brave but vain defence against an overwhelming Turkish force, by whom, in the short space of a few hours, this before-flourishing island was made a desert, in the most literal sense. Though rather more than two years had elapsed since the massacre, when I saw them, the bodies were little changed; indeed, the features were so distinct, though black and shrunk, that our guide (the only living inhabitant in an island which before had an active population, exceeding 15,000) recognised each person and called him by name; even their clothes (they had not been stripped) had experienced little change.

The situation of the place where they fell was dry, and its aspect northerly. In the castle of Ipsara, on the contrary, which had a southerly aspect, nothing but whitened bones remained of the numerous victims of despair, who blew themselves up by exploding the magazine, when the place was taken. These facts strongly exemplify the difference of character of the very dry Etesian, or northerly wind of the Mediterranean, and of the very humid Sirocco, or southerly, and especially in the Archipelago.

† The effect of the sun's rays on the hair of the Negro, is, I believe, analogous to that on the European, &c., but less powerful: I say, *believe*, because I am not quite certain of it.

first, relative to the degree of accuracy which may be claimed for them; and secondly in relation to practical use and application.

As regards accuracy, the results generally can with propriety be considered as approximations only to the truth. The principal sources of error were two—moisture and air, foreign to the composition of the part, both of which it is difficult to remove with that precision which is necessary to insure perfect correctness. The effect of adhering fluid, however, in slight excess or deficiency may, I believe, be held to be of little account in comparison with the other cause, the air, the difference of weight of which, compared with that of solids and liquids, is so enormously great. This source of error exists in different instances in very different degrees, probably very nearly in direct proportion to the vascularity of the part tried, and the size of its vessels, and also to its dryness. This may be deduced from the experiments already related on the action of the air-pump on animal textures. Strictly, in experiments on the specific gravities of bodies, all adhering air, whether superficially attached or contained in the internal structure, should be removed by exhaustion. Whilst I express regret that circumstances did not permit my using this precaution as I could have wished, I shall give a small number of examples in which I have since employed it, from which some judgment may be formed of the amount of error attributable to the presence of air.

The first instance I shall offer is that of the parietal bone of a man of middle age, a case of amentia, which proved fatal in consequence of pulmonary consumption.

Before being subjected to the air-pump, its specific gravity appeared to be 1790 to distilled water as 1000 ; after exhaustion, 1970.* The quantity of air disengaged was very great ; and it required long continued pumping to remove it, indeed, after working the pump for more than an hour almost uninterruptedly, a few particles continued to be disengaged.

A portion of the same bone was calcined. The fixed part, indestructible in the fire, consisting chiefly of phosphate of lime, weighed before being subjected to the air-pump, appeared to be of specific gravity 2580, and after of specific gravity 3070.†

Another portion of the same bone was kept immersed in dilute muriatic acid, until all the calcareous matter was dissolved ; the residual cartilage was well washed and afterwards gently pressed between folds of linen, to remove all the moisture that could then be separated : thus treated, before being

* The bone was dry, prepared for the Museum ; but it contained some hygrometric water. Exposed to a temperature of about boiling water for three hours, it lost 9.64 per cent. water ; making allowance for which, its specific gravity would be, instead of 1970, 2200. This example may serve to illustrate the influence of moisture in ascertaining the specific gravities of animal structures.

† The specific gravity of mineral phosphate of lime—apatite, is from 3100 to 3200.

subjected to the air-pump, its specific gravity appeared to be 1070, and after exhaustion, 1119.

The instances of bone, in its dry state, and of its calcareous residue after the action of fire, may be considered perhaps as extreme ones,—in relation to the source of error in question, the hollow texture of both being peculiarly well fitted to retain air. The next examples I shall bring forward, may be considered perhaps, as rather average ones,—namely, the ligamentum nuchæ of the ox, and the middle coat of the arch of the aorta of the ox. The former, before being subjected to the air-pump, appeared to be of sp. grav. 1130, and after being so subjected of sp. grav. 1134;—the latter, in the first instance, of sp. grav. 1073, and in the second of sp. grav. 1077.

The substance of the brain may be mentioned as an example of a part not subject to the source of error under consideration, for *in vacuo*, as has been already stated, no air is disengaged from it; that is, if immersed in water the instant a portion of it has been cut out a few hours after death,—before the putrefactive process has commenced.

Relative to the practical use and application of the results, I venture to indulge in the hope that the physiologist will receive them as a contribution to his science, considered as a collection of facts and of rules deduced from facts; and also, that the pathologist, may derive from them a help to the eye, and a more certain criterion of organic change

than we yet possess in this important but obscure branch of medical knowledge: but for this latter purpose the investigation will require to be prosecuted further. The quality of specific gravity will, probably, be most applicable, as a characteristic to the viscera,—especially the liver and the spleen; and perhaps, the brain, lungs, and kidneys. The results given of the experiments on the two first mentioned viscera are favourable to this conjecture. These results are very various, and almost demonstrative that organic disease of the viscera cannot take place without altering their specific gravity; and, if so, it is not improbable that this precise quality may be made an index of the kind and degree of organic change. And, *a priori*, perhaps, thus much might be inferred; for if fatty matter is deposited, the specific gravity must be diminished; if cartilaginous matter, it is probable it will be increased; probably, it will be variously diminished or increased according to the kind of deposition. Moreover, attention to these changes may lead to more minute observation, and to the collecting of information curious and useful. Every new means of research is in this way indirectly beneficial. Whoever employs the stethoscope, though he may not appreciate its merits in the same high degree as the ingenious inventor of the instrument did, yet he cannot fail to derive instruction from its use, and to become better, more minutely, more discriminatively acquainted with the diseases of the chest.

The same remark applies to Avenbrugger's method of diagnosis by percussion, and indeed to every other method of investigation, which directs and fixes the attention,—drawing it away from vague and unmeaning generalities, the bane of knowledge, to precise and significant particulars,—which are its essence,—and which are equally important, whether we have in view, during life, the symptoms of disease by the bed-side of the patient; or, after death, on the dissecting table, the organic changes, the effects of diseased action.

The greater portion of the preceding observations, first appeared in print in the 3d volume of the Edinburgh Medico-Chirurgical Transactions for 1828. Recently, on consulting Professor Burdach's Treatise on Physiology, I find that the subject under consideration, has, since that time, been investigated by MM. Schubler and Kapff, who published a work on it, in 1832. Comparing their table of specific gravities of the different textures of the human body, contained in the 8th volume of the treatise on physiology already referred to,—with mine,—some of the results are found to agree, others to disagree, as perhaps might be expected, partly from real differences of specific gravity in the same textures in different bodies, which is strongly exemplified in my list,—and partly from the methods of operating and the interference of minute and obscure causes of disagreement and error, not easily detected, independent of the two main ones

already pointed out. The most striking difference between their results and mine, presents itself in the instance of the lungs: and I select it the rather for comment, as it affords illustration of one of the causes of difference. According to MM. Schubler and Kapff, the specific gravity of the substance of the lung is 645 to water as 1000;* according to the result obtained by me, 1054. I have stated that the specimen which I subjected to experiment was destitute of air: their specimen, of course contained air: and it follows, that as the air in the lung is in variable quantity, its specific gravity also, unless air be excluded, must be variable also.

* *Traite de Physiologie*, tom. viii. p. 6.

IX.

ON THE EFFECTS OF DESICCATION, AND THE PRO-
PORTION OF WATER CONTAINED IN THE DIFFER-
ENT TEXTURES OF THE HUMAN BODY.

THIS subject, like that of the specific gravity of the different textures, may be considered also as elementary and fundamental; for unless it is known with tolerable accuracy what is the quantity of dry solid matter, each part is capable of affording, or may be said to contain, other statical results will of necessity be vague and of little value.

It was in Malta, in the summer of 1828, that I commenced the inquiry, desirous of conducting it on a more extended plan than I believe had been previously attempted, by any one individual; so that, one method being employed, and the circumstances being similar, the results might be compared with confidence, and discrepancies avoided.

The method of desiccation which I employed was simply that of the vapour-bath. The parts were either placed on paper when not likely to adhere, in a Wedgewood evaporating dish, over steam; or

suspended over it almost touching the bottom, by a fine thread, when of an adhesive kind; or, when very soft, they were placed in it, in platina capsules previously weighed. They were kept on the bath till all the water capable of being expelled by a temperature of about 212° , was driven off, which was indicated by their ceasing to lose weight from further exposure.

The results, first published in the 4th volume of the Transactions of the Medico-Chirurgical Society of Edinburgh, are contained in the following Table, constructed on the same plan as the preceding, and in which the Roman minerals are designed for similar reference.

Table shewing the proportion of dry Solid Matter in different parts of the Human Body.

No.	Subject.	Part tried.	Result per cent.
1	i.	Cuticle of sole of foot	51.5
2	iii.	Projecting part of nail of thumb, rather curved as in phthisis	83.3
3	—	Inner part, soft, diaphanous, carefully freed from adhering subjacent layer	76.6
4	—	Bosjesman's hair	91.6
5	—	Hottentot woman's hair	92.0
6	—	Kaffer boy's hair	92.3
7	—	Young woman's hair of Pitcairn Island	91.3
8	—	Light coloured hair of an Englishwoman	90.7
9	—	Dark coloured ditto	90.7
10	ii.	Skin over belly of deltoid muscle	32.4
11	iv.	Ditto over lower part of scapula	35.3
12	v.	Ditto from middle of back	42.3
13	vi.	Ditto, ditto	39.5
14	vii.	Ditto over first joint of thumb	39.0
15	—	Ditto of fore-arm, just below its bend	32.8
16	—	Ditto of shoulder	37.4
17	—	Ditto of scrotum	30.2

No.	Subject.	Part tried.	Result per cent.
18	vi.	Cornea	27.3
19	—	Sclerotica	33.3
20	i.	Fatty matter from beneath integuments of abdomen	88.9
21	—	Pectoralis major muscle	15.6
22	iii.	Muscular substance of left ventricle of heart	20.5
23	—	Pectoralis major muscle	24.0
24	v.	Gluteus maximus muscle	25.8
25	—	Columna carnea from left ventricle	21.2
26	iii.	Cerebrum (centrum ovale)	33.9
27	—	Cerebellum	20.8
28	—	Medulla oblongata	31.0
29	v.	Sciatic nerve	28.5
30	i. ¹	Aorta (inferior abdominal portion)	22.5
31	iii.	Ditto at its origin	28.0
32	v.	Ditto at its origin	34.5
33	—	Femoral artery just below the arteria pro- funda	31.4
34	vii.	Ditto, ditto	34.4
35	—	Aorta just below its arch	31.7
36	—	Ditto just below the cœliac	28.2
37	—	Ditto (its termination)	26.5
38	—	Common iliac artery	24.6
39	ix.	Basilar artery	26.8
40	—	Arteria sylviana	31.0
41	x.	Basilar artery	34.0
42	—	Arteria sylviana	31.4
43	i.	Ascending vena cava	27.9
44	v.	Femoral vein contiguous to arteria profunda	31.3
45	vii.	Ditto a little lower	32.3
46	—	Vena cava ascendens (its commencement)	25.0
47	—	Superficial vein at bend of fore-arm	33.3
48	vi.	Thoracic duct	29.5
49	i.	Substance of liver	22.9
50	iii.	Ditto	24.3
51	v.	Ditto (Lobulus Spigelii)	25.8
52	viii.	Ditto	28.5
53	i.	Substance of spleen	19.2
54	iii.	Ditto	24.3
55	i.	Substance of kidney	19.0
56	iii.	Ditto	18.4
57	i.	Substance of lung	22.2
58	iii.	Substance of pancreas	22.2
59	iv.	Substance of testicle	16.3
60	—	Great arch of stomach	19.5
61	—	Upper part of jejunum	18.6
62	viii.	Ditto	11.8
63	iii.	Dura mater	32.7
64	i.	Pericardium (outer layer and a little fatty matter under it detached)	41.0
65	i.	Perichondrium of cartilage of false rib	35.0

No.	Subject.	Part tried.	Result per cent.
66	iii.	Perichondrium of cartilage of false rib .	35·0
67	vi.	Pleura costalis	31·0
68	ii.	Corpus cavernosum penis	22·2
69	iii.	Extensor tendon of great toe	32·2
70	v.	Tendo Achillis	32·0
71	iv.	Intervertebral substance from last lumbar intervertebral space, cut out in the form of a wedge	26·7
72	v.	Cartilage of knee-joint	28·0
73	ii.	Cartilage of false rib	33·7
74	iii.	Ditto	40·0
75	i.	Superior part of sternum (spongy portion) .	57·3
76	—	Ditto (outer table)	78·3
77	v.	Shaft of tibia (outer layer)	89·2
78	—	Ditto (inner layer)	88·0
79	iii.	Fibrinous concretion from right side of heart	23·0
80	viii.	Ditto from left auricle	17·5
81	—	Venous blood, of specific gravity 1061 .	20·7
82	—	Serum of blood, of ditto ditto 1030 .	11·7

The parts submitted to experiment were taken from the following subjects.

i.—Aged twenty-nine years; admitted into hospital 20th May; died 2d June, of inflammation of the mucous coat of the ileum and colon, ending in gangrene. Dissection seven hours and a-half after death; body slightly emaciated.

ii.—Aged thirty years; admitted into hospital 23d May; died 30th May, of tetanus, following a fracture of the inferior extremity of the radius, and of two transverse processes of the lumbar vertebræ. Dissection sixteen hours after death; body very muscular and fat.

iii.—Aged thirty-six years; admitted into hospital 3d May; died 5th June, suddenly, with tubercular phthisis. Dissection three hours after death; body considerably emaciated.

iv.—Aged twenty-two years ; admitted into hospital 27th April ; died 9th June, of tubercular phthisis, very far advanced and complicated. Dissection seven hours after death ; body extremely emaciated.

v.—Aged twenty-three years ; admitted into hospital 20th May ; died 13th June, from fracture of the cranium and the cervical vertebræ, with effusion of blood on the brain, and softening of the spinal chord. Dissection twelve hours after death ; body not emaciated.

vi.—Aged twenty-two years ; admitted into hospital 1st July ; died 4th July suddenly, as if from syncope, whilst labouring under a febrile attack, connected with ulceration of the mucous membrane of the ileum, and enlargement and softening of the mesenteric glands. Dissection eleven hours after death ; body not emaciated.

vii.—Aged thirty-two years ; admitted into hospital 5th July ; died 13th July, with febrile symptoms, connected with a vast latent abscess in the liver. Dissection sixteen hours after death ; body not emaciated.

viii.—Aged thirty-three years ; admitted into hospital 3d July ; died 16th July, of acute dysentery, terminating in gangrene of the mucous coat of the large intestines with perforation of the colon. Dissection seventeen hours after death ; body slightly emaciated.

ix.—Aged thirty-two years ; very stout and mus-

cular; died suddenly in barracks, of pulmonary and cerebral ecchymosis. Dissection fourteen hours after death.

x.—Aged twenty-three years; admitted into hospital 17th July; died 30th July, of inflamed and ulcerated ileum. Dissection twelve hours after death; body considerably emaciated.

With the exception of the parts taken from the body No. 1., all the others, the instant they were dissected out, were gently pressed between folds of blotting-paper, to remove superfluous moisture or adhering fluids; and in an hour, or two at farthest, were placed to dry on a vapour-bath, as already mentioned. In the exception above alluded to, the parts were kept together, covered over, till the following morning, and then they were washed with water, and subjected to pressure with blotting-paper, previous to drying.

It may be right to state, that all the bodies were of British soldiers, and that they all died in Malta; and, as will appear from the dates, during the hot season; the thermometer during the whole time fluctuated between 80° and 90° .

Of the effects of desiccation on animal textures, remark is hardly required, as they are pretty generally known, and are such as might be expected *a priori*. The brittleness which the operation imparts, and its preventing putrefaction, are two of its best defined and most general effects. But these effects

are only temporary; as soon as humidity is restored, flexibility is also restored, and the tendency to putrefaction and decomposition returns, and apparently unimpaired by suspension. This property of desiccation in animal matter, and in vegetables also, though it has been long applied to economical purposes, for the preservation of articles of food, has not, perhaps, been used so much as it deserves; nor has the principle, I believe, been applied as generally as it might be, to the solution of certain phenomena, strictly chemical, which occur in nature and art. Does it not serve to explain a fact which, on first consideration, seems not a little extraordinary, that meat will remain fresh for many days in Egypt, if hung up in the open air, and exposed to sunshine and wind, in the hottest weather; whilst it putrefies in a few hours, if confined in a close damp place? Does it not afford equally an explanation of the opposite effects in the Mediterranean, on dead animal matter, of the south-east or sirocco wind, and of the north and north-east winds of which a remarkable example has already been given, witnessed in Ipsara? The former wind, almost saturated with moisture, produces little or no evaporation or desiccating effect; whilst the latter, deficient in moisture, and sometimes in an extraordinary degree deficient, occasion rapid evaporation, and powerfully desiccate; the one acting like confined moist air of high temperature, and greatly promoting putrefaction; the other operating like the dry wind of the desert,

though in a less degree, and retarding the change.* May it not, too, serve to account for so many remains of ancient Egypt, especially of Upper Egypt and of Nubia, being preserved to the present times, the natural dryness of the climate having been assisted by various processes of art? And is not proof afforded of the correctness of the explanation, in this region being almost the only one thus spared and exempted from the general doom of slow and certain change, almost amounting to destruction, which is manifest in other countries, where the elements have free play through the intervention of moisture, even in the works of nature?

Looking over the preceding table of results, it is curious to notice what little relation there is between the solidity of a part, indicated by firmness and resistance, and the quantity of solid matter which it yields on desiccation. Who would suppose, *à priori*, that the soft flexible skin contains more of this solid matter than cartilage; or, that the delicate serous membranes contain more than the firm viscera; or (and it is most remarkable) that the liquid blood possesses a larger proportion of it than some of the solids?

* The cooling effect of the wind is one of the simplest and most certain means of knowing its proportional humidity. The cooling effect on a thermometer, the bulb of which is wrapped in moist muslin, of the sirocco wind, varies from 3° to 6° , and of the northerly winds in summer from 12° to 30° . This is the result of my observations in the Ionian Islands and Malta, and it is applicable, I believe, to the winds named, in the Mediterranean generally.

The proportional quantity of solid and fluid matter, constituting the animal body, is a question to which considerable attention has been paid by physiologists; and it is a problem still unsolved. According to some, the proportion of the former to the latter is as one to six; and, according to others, as one to nine. The results of the experiments on desiccation which I have given, including those on blood and serum, may help to furnish accurate notions on the subject;—to shew, in the first place, how extremely difficult it is to solve the problem rigorously, and how impossible it is to solve it generally; and, in the next place, to show that, in the adult at least, the proportion of fluid or of water has been overrated, inasmuch as in the blood itself it does not exceed one-fifth.

As my experiments were commenced unconnected with any speculative views, so I shall endeavour to abstain as much as possible from such views, as they are very apt to lead astray, and in no inquiries more than in physiological ones, and pervert the judgment and even the senses. Deductions must be drawn from experiments with great care and hesitation, and not from one or two, but from many results. The conclusions which may obviously be made from the preceding experiments, it is unnecessary to point out in detail; and any others I would rather abstain from drawing, excepting conjecturally, and for the purpose of leading to farther inquiry. Thus, in the instance of the results of the experiments on the arteries and

veins, it may be asked, do they not indicate that the proportion of solid matter, in the former decreases with the distance from the heart, excepting in the brain, where the arteries are very analogous to veins;—whilst, in the latter, would it not appear, that it observes a contrary ratio, and increases with the distance? Should ulterior investigation confirm this, it will be an interesting fact established in physiology.

X.

ON THE ACTION OF CORROSIVE SUBLIMATE ON THE
TEXTURES OF THE HUMAN BODY.

THE power which corrosive sublimate possesses, of preserving animal substances, has been known for many years, and has been taken advantage of by anatomists, both in the dissecting-room and the museum, to promote the objects of each, as has been well demonstrated by the minute and elaborate labours of Mr. Swan on the Nervous System. The property also which it has of precipitating albumen from animal fluids has been known for a considerable time, and, as a valuable test in analytical chemistry, has been studied with some care. But I am not acquainted with any researches hitherto made to investigate its action on the textures of the body generally. Conceiving that such inquiry was likely to afford results which might be interesting and useful, I applied myself to it in Malta, in 1828. I shall describe such of the experiments which I then made as I believe are to be depended on; with the hope that they may lead to a renewal of an investi-

gation which unavoidable circumstances obliged me to leave in a very unfinished state.

The two first I shall mention, were of a preliminary kind, to test its power of arresting fermentation and putrefaction.

On the 15th August, at a temperature between 80° and 90° , a small quantity of aqueous solution of corrosive sublimate was added to the expressed juice of the grape in brisk fermentation. The fermentation was immediately arrested. The mixture was put by in a bottle loosely corked, not entirely excluding the air. Examined on the 24th February, no further change appeared to have taken place; the liquid was clear, and had the peculiar smell of sweet must, and the sediment seemed unaltered.

On the 10th of August a portion of muscle putrefying was put into a solution of corrosive sublimate. The putrid process was arrested; the peculiar offensive odour ceased. It was then washed and suspended in water. Examined on the 24th February, it was found unaltered—totally free from any unpleasant smell.

On the 15th of April, portions of different textures, taken from the body of a soldier that had died at the age of 36, were immersed in a saturated aqueous solution of corrosive sublimate, after having been gently pressed between folds of filtering paper, and carefully weighed. They were left undisturbed till the 25th May, when they were taken out, examined, and weighed: they were then

replaced. On the 6th July, they were again examined and weighed, first, in a moist state, after being pressed between filtering paper; and, afterwards, dry, having been thoroughly dried on a vapour bath. The following Table exhibits the results; the column under 15th April gives the weight of the portions tried before immersion; and the two following, the weights of the same, after continued immersion.

No.	Texture tried.	April 15.	May 25.	July 6.	Dried.	Dry matter per ct.
		grs.	grs.	grs.	grs.	grs.
1	Costal cartilage -	17·8	18·6	18·7	7·7	43·2
2	Intervertebral substance -	19·6	19·6	19·2	5·4	27·5
3	Rib -	17·2	14·2	13·2	7·9	46·2
4	Upper part of spinal cord -	14	14·4	13·5	5·3	37·8
5	Pectoralis major muscle -	45	26	27	12·6	28
6	Liver -	23	20·4	19·4	8·4	36·5
7	Spleen -	26	18·5	17	7	27
8	Dura mater -	9·8	8·9	8	3·05	31
9	Pia mater -	4·2	3·4	3·1	1·5	35·7
10	Lung -	19·8	12·5	11·6	4·6	23·2
11	Cellular structure -	2·9	2·4	2·4	1·3	44·8
12	Fatty matter -	6·6	6·3	6·6	5·8	88
13	Kidney -	40	30	24·3	6·9	17·2
14	Pancreas -	25	15·2	14·7	6·1	24·4
15	Fibrinous concretion -	10·3	8	7·4	3·7	36
16	Vena cava -	14·2	11·5	10·6	3·9	27·4
17	Aorta -	10·1	8·2	8·1	3·2	31·6
18	Corpus cavernosum penis -	15·8	9	—	3·3	20·9
19	Cuticle of sole of foot -	6·4	16·6	—	4·3	67
20	Cutis vera -	11·9	10·5	—	4·3	36·1

It is noted down at the time, that when the different parts above-mentioned, were examined on the 25th of May, they were all comparatively pale, appeared to be condensed, and emitted no unpleasant smell, and were perfectly distinguishable: a little calomel had formed on the sides of the glass-vessel in which the experiments had been made. And, also, that when examined, on the 6th July, they appeared to be exactly in the same state. The solution itself seemed to be but little altered; it contained only a very trifling sediment. Evaporated to dryness, it yielded a fawn coloured residue, which, strongly heated, first gave off fumes of corrosive sublimate; next, whiter and denser fumes, as if of calomel; and, lastly, an ammoniacal vapour: a small quantity of carbonaceous matter remaining.

On examining the subjects of the experiments, after they had been dried, with a powerful lens, minute crystals of corrosive sublimate were perceived in several of them, principally in those the weight of which was most increased. To separate the loosely adhering salt, they were immersed and left in water, from the 8th to the 15th July, during which time the water was frequently changed. They were then again weighed, after thorough drying on a vapour bath. The following Table exhibits the results.

No.	Texture tried.	Dried.	Dried matter per ct.
1	Costal cartilage - -	7.2	40.45
2	Intervertebral substance - -	5.1	26
3	Rib - -	7.5	43.8
4	Upper part of spinal cord - -	4.8	34.3
5	Pectoralis major - -	11.3	25.1
6	Liver - -	7.9	34.3
7	Spleen - -	6.4	24.6
8	Dura mater - -	2.85	29
9	Pia mater - -	1.45	34.5
10	Lung - -	4.2	21.2
11	Cellular structure - -	1.3	45
12	Fatty matter - -	4.5	68.2
13	Kidney - -	6.2	15.5
14	Pancreas - -	5.8	23.2
15	Fibrinous concretion - -	3.1	30.6
16	Vena cava - -	3.5	24.6
17	Aorta - -	3	29.7
18	Corpus cavernosum penis - -	3.1	20
19	Cuticle of sole of foot - -	3.6	56.2
20	Cutis vera - -	4.1	34.4

It may be remarked, that none of the textures during this second immersion in water, showed the slightest signs of putridity or tendency to decay; this indeed might have been expected, considering the result of severer trials. The white parts generally, especially the cutis, blood-vessels, and membranes absorbed water, and became more or less flexible, well adapted to display their form and texture in a class-room. The portions of lung, kidney, and liver remained firm; the pancreas and muscle were softened and rendered flexible in an intermediate degree.

Comparing the results contained in the last Table with those in a former Table, shewing the effects of desiccation, it seems very doubtful if the corrosive

sublimate combines with the textures generally, or with any one texture entirely. Probably it unites only with a part of each, that part on which putrefaction chiefly depends, and which it may be conjectured, is essentially albuminous, and not very different in its nature from the albumen ovi, for which the chloride has a strong affinity. The proportionally increased weight of the fibrinous concretion, of the liver, and muscle, is in favour of this conclusion.

It may be asked, does corrosive sublimate combine directly with albumen—as from the manner of expressing my opinion in the last paragraph, seems to be taken for granted? My belief is, that it does so combine. This was the original view of Dr. Bostock, as stated in his ingenious paper on animal fluids, published in Nicholson's Journal for June 1806, in which it is also taken for granted as most natural, and in accordance with facts. It appears to me preferable to the later conclusion of M. Orfila, that the compound precipitated from white of egg by corrosive sublimate, consists of calomel and albumen; and preferable also to the more recent conclusion of Rose, that it is composed of albumen and oxide of mercury. Orfila drew his inference from the products of the destructive distillation of the compound,*—results ill adapted for forming an opinion on the subject. Moreover, at that time, viz. in 1814, the old hypothesis respect-

* *Traité des Poisons*, tom. 1.—Part 1. p. 47.

ing the compound nature of chlorine still prevailed in Paris. The data on which Rose reasoned I am not acquainted with; I have not seen his original paper,—merely a reference to it in the edition of Dr. Turner's Elements of Chemistry for 1834.

The facts which induce me to adhere to the early opinion of Dr. Bostock, are chiefly these:—

1. When corrosive sublimate and albumen ovi combine, there is no disengagement or production of muriatic acid, as might be expected,—whether it contained calomel or oxide of mercury. In one experiment in which the whites of two eggs were mixed with a drachm of corrosive sublimate, and after coagulation at a temperature of about 120° —washed with a small quantity of water, and filtered, the water which passed through the filter, had no well marked acid property, and did not occasion any effervescence when mixed with a solution of carbonate of potash.

2dly, I have not been able to combine calomel and albumen. When I have triturated together the white of egg and calomel, the latter has become grey, almost black as if from the formation of a little protoxide of mercury from the action of the alkali contained in the white of egg: but the white of egg itself has undergone no change.

3dly, and lastly, The attempts I have made to combine albumen and oxides of mercury directly, both the protoxide and the peroxide have been equally unsuccessful.

The properties too, of the compound appear to me most favourable to the idea which I have advocated of its nature. Its properties are strongly marked, and are indicative of energetic union,—being tasteless, insoluble in water, and not decomposable either by the muriatic acid, or by an alkali, or by lime; whilst it is soluble in an aqueous solution of the former, whether of soda, potash, or of ammonia, and also in a slight degree in the liquid albumen ovi.

Dr. Bostock from his experiments, concluded that the compound of albumen and corrosive sublimate contains from $\frac{1}{3}$ to $\frac{1}{4}$ th its weight of the latter. From various circumstances, it is difficult to ascertain the exact proportions of the two, as he himself has pointed out. The estimate can only be considered as an approximation; I mention it chiefly in application to the preceding results.

These results practically considered, are not without interest, as confirming the general antiseptic power of corrosive sublimate,—preventing, it would appear, putrefaction from taking place in every instance in which it was tried,—arresting it when in progress, and equally arresting vinous fermentation.

Mr. Swan, in his “Account of a new method of making dried Anatomical Preparations,” published in 1820,—which I have only recently seen,—has shewn how usefully corrosive sublimate may be employed in the dissecting-room. In consequence of its great antiseptic power for the purpose of carrying

on minute dissections, and for making dried preparations. He employed it in solution in spirits of wine, (2 oz. of the chloride to 16 oz. of spirit of wine) or in water with the addition of muriate of ammonia (2 oz. of the chloride, 70 grains of the muriate and a pint of water). The latter is recommended by its cheapness. For the details of Mr. Swan's method, I must refer to his work on the subject, which is very deserving of the notice of the practical anatomist.

Corrosive sublimate in solution has also been employed for preserving moist preparations; but hitherto not successfully. In 1823, I saw several preparations preserved by means of it, in the museum of the late Mr. Brooks, and in the collection of Mr. Carpue. Mr. Brooks informed me that after steeping for a certain time the preparation in an aqueous solution of the chloride, he transferred it to proof-spirit, covering the jar in the usual manner. All the preparations which he shewed, were more or less injured by a white deposit encrusting them: and in some instances, the insides of the jars were so encrusted as to have become quite opaque, hiding completely the object within. From some experiments which I then made on two preparations which had there suffered, which he was so good as to entrust to me for examination, — I found the encrusting matter to be chiefly calomel,—whether on the preparation itself, or on the interior of the containing vessel: the latter de-

posit was in one instance mixed with a little fat and other animal matter.

From a set of experiments which I made on corrosive sublimate in 1822, and which were published in the Philosophical Transactions for that year, I shewed, that under certain circumstances of complicated affinity, corrosive sublimate is very apt to be decomposed by exposure to light and calomel precipitated; and that the effect may be prevented by adding a substance which has a strong affinity for the chloride, such as muriatic acid, or muriate of ammonia, in a saturated solution of the latter of which, it was, I found, seventeen times more soluble than in water alone.

Such complicated affinities I have no doubt were exercised in occasioning the precipitation of calomel, in the instances of the spoilt preparations to which I have alluded: and it is highly probable that the bad effects might have been prevented by certain precautions, tending to oppose those affinities, as by immersing them after steeping in the chloride solution in dilute muriatic acid, or in a solution of muriate of ammonia; or placing them immediately in a compound solution of the muriate and chloride. The few experiments I have made of this mixture have given favourable results; but they have not been carried sufficiently far, or made with sufficient precision to enable me to give a decided opinion on its *permanent* efficacy. This I regret, considering the cheapness of the means, and the promise of

success which the method holds out. By *permanent*, I mean not a duration of a few months or even of a few years, but an unlimited time,—in relation to use for the preservation of valuable preparations in the museum. To determine this, trials on things of little value ought to be made, with a careful record of dates and effects. With this design, a series of experimental preparations have recently been put up in the Pathological collection at Fort Pitt, and will I hope hereafter afford data for arriving at the information required.

XI.

ON THE ACTION OF LIME ON THE TEXTURES OF
THE HUMAN BODY.

It is commonly asserted and believed, that lime exercises a corroding destructive influence on animal matter in general,—and that animal bodies, exposed to its action, rapidly decompose and disappear. Accordingly, it has been almost invariably recommended, to add this earth to graves, in instances in which a rapid decay is considered desirable,—as on the occasion of the crowding of grave-pits, with dead bodies, during the prevalency of pestilential diseases.*

* The following instance, extracted from the ninth volume of the Philosophical Transactions, abridged, may be given as an example of the vague, and, as I believe, erroneous manner of considering the operation of lime. In 1746, when means for preventing the infection of an epidemic disease, which then prevailed among the cattle, were under consideration, burying them was thought the most effectual method, and the introduction of lime was recommended “for the more speedy destruction of the distempered carcasses.” But doubts arising “whether the lime might not exalt the putrid particles, and help to spread the infection, it was the opinion of several of the learned, that it was most safe on that account to bury them without it.”

Dr. Parsons, the author of the paper, adds, “that the question will

From the results of many experiments which I have made with lime, on animal substances, I have been compelled to come to the conclusion, that this opinion is not well founded in fact,—indeed, that it is altogether erroneous.

The experiments were commenced in Malta, in the summer of 1829, and they were carried on during the following year. The method observed, was to immerse the animal matter for trial, in cream of lime, or rather a paste of lime, contained in a wide-

probably be decided by a fact that had come to the knowledge of one of the justices, John Milner, Esq., appointed to inspect into the affair, and will serve to prevent the practice of burying them with lime for the future, as it makes it more than probable that malignant particles (by the operation of the lime) may be sent up, and spread through the air."

The fact referred to was the following, "Mr. Stallwood, a farmer, at Hackney, informs the justices to whom the care of the distempered cattle was committed; that he had buried thirteen cows, very deep, with the quantity of lime appointed by the justices; and, observing his dogs to scratch, and tear up the ground with their feet, to get at the cow's flesh, (the lime fermenting and causing a foam as he called it, or strong scent of meat to arise, which made the dogs so eager to come at it), he beat them off several times; but the dogs always returning, as soon as he was gone, for some time he hired a boy to keep them off. But that he had buried several other cows in another place, with their hides cut and slashed, without any lime, and the dogs never attempted to scratch or tear up the ground there." Two bushels of lime were allowed to each cow. With lime, the bodies were buried ten feet deep; without lime, eight feet deep.

Relative to the explanation of the fact,—was not the difference observed in the two instances owing to this,—that in the one the dogs were attracted by the smell of the meat preserved by the lime,—and not in the other, where it was not so preserved, and where it was undergoing putrefaction?

mouthed bottle, well corked, and covered with cerate cloth, to exclude the ingress of atmospheric air, and so preserve the lime in its caustic state.

One of the first experiments tried was commenced on the 27th August. A portion of muscle, rectum, verge of anus, urinary bladder, prostrate gland, vesiculæ seminalis, a portion of jejunum, the gall-bladder, and the gall-ducts, were immersed as mentioned above. They were taken from a subject in a state of incipient putrefaction, and they exhaled a fetid smell. On immersion in the lime and water, as might be expected, they gave off a strong ammoniacal odour. They were first examined on the 24th of September. They were then all in excellent preservation; swollen, but not corroded, nor their delicate tissue injured. They were next examined seven months after, viz., on the 5th of May of the year following. The report was equally favourable; it is stated that they were much in the same state as before,—the texture of each part distinct,—and the part, as a whole, easily distinguishable. They were left undisturbed nearly two years,—until the 6th April, 1832, when, on examination, they were found to have undergone material change. The cuticle had become soft and transparent, as had also the dura mater, admitting of being torn with the greatest ease. The muscle appeared to be converted into adipocire, which was quite white,—had no unpleasant smell,—was friable when dried, and

burned with a bright flame, without any unpleasant smell. The other parts were not distinguishable.

The second experiment recorded, was commenced in the beginning of October. Portions of aorta, dura mater, intestine, skin, cellular tissue, muscle and tendon, were similarly treated. The results were examined on the 5th of May following. Then, on opening the bottle, an ammoniacal but no putrid smell was perceptible. The parts were found well preserved, excepting the fatty matter contained in the cellular tissue, which had become of an opaque white and friable, from combination with the alkaline earth and conversion into soap. The tendon, it is mentioned, was somewhat distended and rendered more transparent, but not gelatinized; and so also, in a less degree, were the dura mater and cutis; and the last was deprived of its cuticle and hair.

Some other experiments were made, but, as the results were very similar, it would be tedious to describe them. I may state, generally, that with the exception of cuticle, nail, and perhaps hair, lime exerted on the different textures, on which it was tried, no destructive power, but, a contrary influence,—and more particularly a well marked anti-septic one. It has been stated, how certain parts in the first experiment, lost the putrid odour which they had acquired, when immersed in lime and water. Moreover, it appears from notes of experiments,

that after animal substances have been fully subjected to the action of lime, they ceased to be putrescent ; they resisted putrefaction, whether placed in air, or plunged and kept in common water. I shall mention one instance. On the 13th of May, 1830, a portion of ileum, with mesentery attached, and a portion of muscular part of heart, with chordæ tendineæ, were placed in a large jar of transparent lime water, and covered with cerate cloth. Examined nine months after, on the 16th of February, they were found in good preservation, and without any putrid or unpleasant odour. The only change perceptible was, that the portion of heart and intestine had acquired a light greenish hue and the tendon an opalescent hue ; and all were a little softened. A crust of carbonate of lime had formed on the water, which still retained some caustic lime. They were then transferred to a jar of common water—where, after four days, they continued unaltered. I may add, that a portion of cutis, similarly treated, placed in confined air in a bottle, after a whole month, emitted no unpleasant odour, and appeared to be unchanged.

I have observed, that cuticle, nail, and perhaps hair, are to be excluded from the list of animal substances, not materially altered by the action of lime. On the cuticle its action is powerful, and, I apprehend, in consequence of a chemical combination between them being formed. It is well known how lime has the property of rendering the cuticle easily

separable from the cutis vera, and how, in the art of tanning, it is applied to this purpose. The human cuticle, that, for instance, of the sole of the foot, I find, becomes soft and gelatinous from immersion in lime and water. After drying, a portion thus tried, (well washed previous to drying) was white, semi-transparent and brittle; incinerated, it yielded seventeen per cent. of ash, which consisted principally of lime and carbonate of lime.

The effect of lime on nail is similar to that which it exercises on cuticle, but not so strongly marked. A portion of nail of great toe, macerated in lime and water, from the 7th of June to the 18th August, was rendered soft and friable,—a little swollen and disposed to separate or break up in layers. Dried, it exhibited the same character as cuticle, and when incinerated, burned in a similar manner, and left a considerable ash, consisting of a small proportion of phosphate of lime, which pre-existed in the nail, and a large proportion of lime,—with which, during the change from maceration, it may be inferred, it combined.

On hair, the effect of lime appears to be more destructive; but, in what manner it acts, I have not attempted to ascertain. A portion of human hair of the head, which had been kept in lime and water about three months was partially decomposed. At the bottom of the vessel there was a little black sediment. The hair, which was black, had acquired a just perceptible reddish shade, and had become

much finer, as if wasted, and more feeble, so as to be easily broken.

Relative to the results of the experiments generally, they appear to me to bear me out in the remark with which I prefaced them, viz., that lime does not exercise a destructive corroding power on animal substances generally, or one promoting their decomposition ; but, on the contrary, a preservative and decidedly antiseptic power, arresting putrefaction, even when commenced, and retarding decomposition. What new arrangements of the elements of animal matter may take place under the influence of lime, is a subject for further inquiry. Probably the effects of lime on cuticle, nail, and hair, on which in the arts its operation has been best known, led to the ideas of its agency on animal substances generally, which I have been under the necessity of combating.

It was my intention to have instituted a set of experiments on the effects of the other earths, and also of the alkalies on animal textures ; but this I have only very partially accomplished.

From the few trials which I have made with barytes and magnesia, the action of the former on animal substances generally, and also on the cuticle and nail appeared to be similar to that of lime, but more energetic ; whilst that of the latter appeared very much less, not arresting the putrefaction of the soft parts, and having, if any, an obscure and slight effect on cuticle and nail.

The effects of the alkalies, as might be expected, were sufficiently well marked.

Ammonia, in the form of aqua ammoniæ, acted very like lime. Portions of skin, of sole of foot, nail, hair, intestine, dura mater, after having been a fortnight in this liquor, had very much the same appearance, as if they had been immersed in lime water. The cuticle was detached; it was quite pulpy; it offered no resistance to pressure, and in consistence was not unlike starch gelatinized by cooling. The other parts, not including the nail and hair, were rather soft and swollen, but not materially changed; the hair and nail shewed no alteration, with this exception in regard to the latter, that its inferior stratum—its under surface, was softened, like cuticle. After another fortnight, the cuticle was found to be quite dissolved; the other parts, including nail and hair, remaining as last noticed: nor after another month were they found further altered, excepting that they were softer, especially the skin, which had become very flexible, and no longer retained the hair.

Weak solution of potash, in the form of aqua potassæ, tried on the substances last noticed, had very little effect on any of them; it prevented indeed their putrefaction, and softened them, but no more; after a fortnight, they appeared unaltered, and the cuticle was not detached.

A strong solution of potash, formed in the proportion of a dram of the hydrated alkali to an ounce

of water, acted powerfully on them; the cuticle was first rendered gelatinous, and afterwards reduced to a blackish powder, as if entirely decomposed, and after about a month's action on the other parts, they too had disappeared, a little viscid mucus-like matter having taken their place, and a black matter subsided. These few experiments on ammonia and potash were made in Malta, in the summer season, at a temperature between 80° and 90° .

XII.

ON THE ACTION OF LIME ON VEGETABLE SUBSTANCES.

REASONING from analogy, from what I had witnessed of the effects of lime on animal substances, I was induced to question the views which are commonly entertained of the operation of this earth on vegetable matters,—as its supposed power of facilitating their decomposition, and promoting their fermentation and solution. And the few experiments which I have made have more than confirmed me in my doubts.

As the subject is of very great importance in relation to agriculture, I shall describe the results which were obtained, using small quantities as most manageable and best accordant with my limited means, hoping that my statements may induce others to repeat the experiments on a large scale, and extend them in a manner befitting the consequences involved.

The experiments were commenced in June 1836, and they were concluded in November 1838. I shall describe them individually.

On the 27th of June 1836, one portion of saw-

dust, I believe of Norwegian fir, was put into a bottle, with distilled water and quick-lime (the bottle was about half-filled with the mixture), and corked.

Another portion of saw-dust was put into a bottle with water, and corked, but without the addition of lime.

Examined on the 1st November 1838, the saw-dust with the lime had no appearance of any material change; its colour perhaps was a little heightened; the water only just perceptibly coloured; it had a strong taste of lime; evaporated to dryness it afforded a light yellow residue, consisting chiefly of lime; the proportion of vegetable matter was hardly appreciable.

The other portion examined, after the same interval, also exhibited very little change; a mucilaginous film had formed over the submerged stratum of saw-dust, too delicate and small in quantity to be collected and examined in a satisfactory manner; the saw-dust retained its colour, and the water was colourless. The water, evaporated to dryness, yielded a very minute brownish residue, slightly bitter, which had no effect either on litmus or turmeric paper. No smell was perceived on opening the cork of either bottle.

On the 17th June, 1836, some clover leaf and flower, and some leaf of the common mallow were put into a bottle with quick lime and water; the bottle was corked and the cork was covered with sealing wax. The quick lime used was tested for

carbonic acid, and found to be perfectly free from it,—not effervescing in the slightest degree in dissolving in an acid.

For comparison, two other mixtures were at the same time made of the same vegetables and water, and bottled,—one of which was corked, being about half full of air; the other was not corked, a little cotton-wool merely was put into the mouth of the bottle to exclude dust and prevent evaporation.

Examined on the 1st November 1838, the mixture with the lime, appeared to be but little altered; the leaves and the flower retained their form; the former were of a bright fresh green; the latter had become brown; the water was colourless; the lime had acquired a light greenish hue; it dissolved in dilute muriatic acid without giving off a particle of carbonic acid gas. The water evaporated, yielded but a small residue, consisting chiefly of lime. The leaves and flower though they retained their form, yet when shaken in the bottle, were broken into small pieces, and the leaflets detached. Farther, it may be remarked, that the smell perceived on drawing the cork, was similar to that of bruised clover.

The other mixtures to which lime had not been added exhibited different results. That which was corked, a portion of air included, examined on the 4th November 1838, appeared much altered; there was at bottom a light greenish sediment,—at top an almost black mass, and suspended in the water in-

intermediately the small flower leaves, almost colourless; the water was greenish, and when evaporated to dryness, yielded a small brownish extract; the leaves were in a pulpy state and disorganized; the smell from the mixture was offensive, not unlike that of clover fermenting.

The mixture from which air was not excluded, fermented soon after it was put by; ten days after, viz. on 27th June, it was noted, that the vegetable matter was rapidly decomposing, with a very offensive odour, approaching to that of the putrid; that much gas had been disengaged, and a good deal of sediment had collected.

Examined on the 2d November 1838, it was found in a state very similar to that last described, bearing marks of advanced decomposition.

An experiment similar to that on the mallow leaf and clover, was made with lime and water, on moss and lichen, and with a very similar result. It was commenced on the 23d June, 1836, and terminated on the 1st November, 1838. The moss and lichen retained their form, and bore being shaken in the bottle without falling to pieces. The water had acquired a light greenish hue; the lime a brownish hue; the water evaporated afforded a small brownish residue, consisting chiefly of lime.

I shall mention one experiment more of the same duration, in which clover leaf and flower, and mallow leaf, were put into a bottle covered with hydrate of lime, and the access of air excluded by a cork and

sealing wax. Examined on the 2d November, 1838, the flower was found brown; the clover leaves of a very light green; the mallow of a dark green, and all very friable, falling to pieces when touched. The lime was examined, both before and after, for carbonic acid; it was quite free from it at the commencement of the experiment, as it was also at the conclusion; it dissolved in an acid without the slightest effervescence, and appeared to be quite unaltered.

These results appear to me conclusive, that lime does not promote the decomposition of vegetable matter; and, as I have before mentioned, that instead of promoting, it arrests its fermentation. The circumstance that no carbonic acid could be detected in the lime after having been in contact with vegetable matter,—both with and without water,—I apprehend, may be considered as demonstrative on this point.

Whether lime has any solvent power on vegetable matter, apart from the supposed one of exciting fermentation, is a distinct question. From what I have witnessed in carrying on these experiments, I infer, that it has, in a slight degree, at least, in combination with water. The extract obtained by evaporating the lime-water, which was in contact with the vegetable matter, was, perhaps, indicative of this, especially in the instance of the saw-dust; as was also the softened state of the leaves and flowers,—falling to pieces on being shaken,—and confirmation, perhaps, is afforded in the results of two com-

parative experiments, made on the same leaves and flower, with magnesia and water, and a solution of carbonate of potash (the old sub-carbonate). Both mixtures were made on the 23d June, 1836, and they were both examined on the 4th November, 1838. There was a marked contrast between them. The leaves and flower, with the magnesia water, retained their form unaltered, and their texture did not appear to be materially weakened,—they bore being shaken in the bottle without falling to pieces, and the water was only just perceptibly coloured greenish, and the magnesia brownish, the leaves retaining their colour unimpaired. The leaves and flowers, on the contrary, in the alkaline solution, were reduced to small pieces, and seemed to be wasted and deprived very much of their colouring matter, and in a pultaceous state; the solution was of a dark olive green; evaporated, it yielded a residue abounding in colouring matter. Lime, in its solvent power, is probably intermediate in degree, between magnesia and the more active alkali—more active even in combination with one proportion of carbonic acid, than the magnesia, or even lime in a caustic state.

The application of the preceding results to agriculture, in relation to manures, I must decline discussing; the subject is one of too much importance, and magnitude, and difficulty, to be lightly entered on.

XIII.

ON THE EFFECTS OF TANNIN ON THE DIFFERENT
TEXTURES OF THE HUMAN BODY.*

THE question, are there any other textures besides the cutis, which have the power of combining with tannin, led me, whilst stationed in Corfu, and in Malta, in 1837-38, to institute some experiments on the subject, believing that there would be no difficulty in arriving at satisfactory results, and hoping, that even unexpected results, as so often happens in experimental research, might be obtained, which might prove both curious and useful.

The first set of experiments I undertook, was commenced on the 1st of November, of the year already mentioned, at Corfu. The parts employed were from the body of a young man, who had died of remittent fever; they were kept in a tanning infusion until the 23d of June. The second set of experiments entered on, was commenced in Malta, on the 15th of April following. Most of the parts

* First published in the fourth volume of Transactions of the Edinburgh Med. Chir. Society.

employed were from the body of a man, aged thirty-five years, who had died from the effects of a severe injury, occasioned by a fall; they were kept immersed until the 4th of July.

The parts subjected to the experiments, the instant they were separated from the body, were deprived of superfluous moisture, by pressure between folds of blotting paper, and then carefully weighed, and immediately after put into a strong infusion of catechu, with some catechu in powder. In every instance, putrefaction to a certain extent took place; after a while it diminished and ceased. The infusion was changed once or oftener in the course of the experiment, according to circumstances; or, if the part was very small, not at all. The experiments were continued till a full effect appeared to be produced; to ascertain which, the parts were occasionally taken out and examined, and partially dried by blotting paper, and weighed. The following table shews the quantity of solid matter per cent. which each afforded, when dried on a vapour-bath, as in the experiments on desiccation. The increase of weight, it was supposed, when it could be clearly established, would serve to indicate the quantity of combined tannin.

No.	Part tried.	Residue per Cent.
1	Skin of thumb, afforded	55·6
2	Cuticle of sole of foot, most of the cutis removed	53·4
3	Pleura costalis	83·3
4	Lower part of ileum	38·5
5	Upper part of jejunum	34·4
6	Aorta	52·3
7	Muscular part of diaphragm, neither its peritoneal or pleural lining removed	55·3
1	Rib, afforded	58·8
2	Cartilage of false rib	57·0
3	Intervertebral substance	41·7
4	Dura mater	53·0
5	Pia mater	53·4
6	Cellular structure	69·3
7	Perichondrium of cartilage of false rib	72·2
8	Cornea	55·5
9	Sclerotica	70·0
10	Corpus cavernosum penis	45·7
11	Medulla oblongata	43·4
12	Pectoralis major muscle	36·4
13	Aorta	47·1
14	Vena cava	40·2
15	Thoracic duct	53·5
16	Liver	27·6
17	Spleen	28·3
18	Lung	23·1
19	Kidney	17·4
20	Pancreas	16·4
21	Fibrinous concretion from ventricle of heart	45·6

I have stated, that putrefaction took place in every instance, in a greater or less degree, and that after a time, on changing the infusion, it ceased. Some animal matter, too, in each instance, was separated from the part, and deposited. At the same time the infusion of catechu had its strength impaired; some of the catechu appeared to be precipitated in the sediment, a part of it in most instances seemed to combine with the texture, and another part of

it rose to the surface, in the form of a thick pellicle, having undergone some change.

The animal parts which seemed to suffer most from putrefaction, were the liver, spleen, pancreas, and kidney. The cuticle was the only part that did not appear to combine in the smallest proportion with tannin, or to be preserved by it. After long immersion in the infusion, it was as it were rotten, the particles of it hardly held together, and were most easily detached, and, when dried, their cohesion was almost entirely destroyed. This, however, strictly applies only to the outer layer, not to the inner, and there appeared to be a gradation from it. The nearer it approached the cutis the more coloured it had become and compact, and the more it seemed to partake of the nature of this membrane, when converted into leather. All the other parts were more or less indurated, and rendered more compact and firm, and all of them were more or less coloured. When dried, they were all comparatively rigid, with the exception of the pleura and pia mater. When immersed in water, they imparted to the water a brown colour. The colouring matter thus withdrawn when exposed to the air, was gradually precipitated, and became insoluble like vegetable extractive, and when burnt, it emitted a mixed fume of vegetable and animal matter, — the former predominating; and, by its abstraction, though the colour of the different textures was very little altered, yet they were all ren-

dered more brittle, even the pleura, when dry, lost almost entirely its flexibility. I tried the effect of water in this manner only on the parts employed in the first set of experiments. The loss each part sustained, per cent., when dried on the vapour-bath, was as follows :—

No.	Part tried.	Per cent.
1	Cuticle of sole of foot	21·8
2	Skin of thumb	7·3
3	Pleura costalis	25·0
4	Lower part of ileon	9·7
5	Upper part of jejunum	9·0
6	Aorta	6·0
7	Muscular part of diaphragm	3·4

The great loss sustained by the cuticle was probably chiefly in consequence of the tannin and colouring matter which it had merely imbibed, when distended by the infusion like a sponge, being washed out, and from some of its particles having been detached, in consequence of its decayed state already alluded to. None of the parts shewed the least tendency to putrefy, during this immersion in water, though the temperature of the air at the time was high, often above 90°, and they were exposed to the air.

Considering all the circumstances of these experiments, the loss of a portion of animal matter from putrefaction,*—the apparently great increase

* I believe the degree of putrefaction and effect produced by it, is in some measure proportional to elevation of temperature, and that

in weight in several instances, and the change of properties produced in all, tending to the preservation of the part, with the exception of the cuticle, I think it may fairly be inferred, that the cutis is not solitary in its power of combining with tannin and forming leather; on the contrary, that the fibrin of the blood, in the form of a fibrinous concretion, or the buffy coat; and that the fibrous and serous textures, and the vessels, including each kind, possess this power in a still more eminent degree,—and that the mucous membranes possess it in an equal degree; and may not the same conclusion be extended to muscular fibre? Relative to the liver, spleen, pancreas, and kidney, their power of combining with tannin is problematical. That a portion of each of these viscera does combine with it and the colouring matter of catechu, is pretty certain, and most likely it is the connecting cellular structure which enters so largely into the composition of their substance. I am disposed to think that this remark does not apply to medullary matter; it seems in a certain manner to be preserved by tannin, without entering into distinct combination with it.

After having obtained the preceding results, the

this is a principal cause that the leather is so well fitted for the climate in which it is made. Made in a hot climate, it is soft and very porous, allowing heat and perspiration to escape; made in a cold and temperate climate it is strong and dense, excluding moisture and retaining heat.

idea naturally occurred, that other textures of animals besides the cutis, might be subjected advantageously to the process of tanning for various uses. It seemed not improbable that some parts might be useful in the form of tubes, as the intestines, thus converted into leather. Farther, it seemed highly probable, that the art of tanning might be applied to the making of certain anatomical preparations, in a dry state, for the purpose of public demonstration, and that it might also be turned to account in the art of embalming. The few experiments which I have hitherto made in relation to these applications, are as favourable as could be expected, and make me sanguine, that whoever undertakes to push them farther, and give them a fair trial, will be eminently successful. Some preparations which I have made of the larger blood-vessels and of the intestines, display their parts perfectly in a dry state, and when immersed in water and distended by the absorption of this fluid, they even display very well the different coats of which they are formed. I suspect that, anciently in Egypt, tannin was employed in embalming. Some years ago, I recollect having examined a heart, and the fluid in which it was found preserved, in an alabaster vase, lined with a substance like petroleum. It had been brought, I believe, from Thebes, in Upper Egypt, and, if I am rightly informed, it is deposited by Dr. Bree, to whom it belonged, in the Museum of the College of Physicians of London. The

heart had the appearance of being tanned, and the fluid in which it was preserved was chiefly water, holding in solution some animal matter and a vegetable principle, analogous to tannin, both in very small quantity. It is probably the most ancient moist preparation known, and it is well adapted to show the preservative powers of a fluid; and that if air be excluded, there is scarcely any limit to the durability of bodies so protected.

XIV.

ON THE EFFECTS OF BOILING WATER, AND OF BOILING, ON THE TEXTURES OF THE HUMAN BODY AFTER DEATH.

THE majority of the experiments which I have to detail on this subject, were made in Malta, in 1828–29. They were instituted chiefly for the purpose of testing, in part, the correctness of Bichat's theory of membranes,—in the development of which, in comparing different textures, he lays much stress on the effects in question.

I shall first notice the effects witnessed from the application of boiling water to portions of different textures, of small bulk, so small as to have their temperature almost immediately elevated to that of the scalding medium. The trials were made in a very plain and easy manner. In each instance, the animal matter was placed in a Wedgewood-ware evaporating dish, and water boiling from the fire, in a common tea-kettle, was poured on it, in a continued stream, for a few seconds, until its own temperature, it was supposed, was imparted.

1. Cerebrum, medulla oblongata, deprived of their

membranes, did not appear to contract or undergo any change of form, neither the cortical or medullary part: they were rendered, however, weaker, as if the cohesion of their particles was diminished,—giving way when an attempt was made to raise a portion with the forceps; and yet they were not sensibly softened.

2. Muscle; a portion of gastrocnemius contracted pretty much, and apparently in all its dimensions. Portions of other muscles were tried, and with like effect. At the same time, that they appeared to be condensed, they became comparatively pale; and also rather hard, seeming to retain their strength of fibre.

3. Liver, spleen, kidney, lung; each of these experienced contraction, and all of them became friable. The portion of lung, when covered with hot water, quite effervesced, so much air was disengaged, partly, no doubt, from the expansion of the contained air from heat, but, chiefly probably from contraction of the air-cells.

4. Cartilage; a superficial slice of the investing cartilage of patella, contracted pretty much and became bent, forming nearly half a circle; the next slice contracted and was bent much less, giving rise to the idea, that the greater effect on the former was owing to its possessing a delicate membrane, liable to greater contraction from heat, than the substance of cartilage alone.*

* There is a preparation, in the Museum of the Army Medical

5. The tendo achillis became shrivelled, and appeared to contract in all its dimensions.

6. The dura mater became corrugated and appeared to be much contracted; from shining silky white, its natural colour, it became dull white, with a yellowish tint.

7. The pericardium became shrivelled and appeared to be contracted; its inner surface exhibited corrugation, and seemed distinctly fibrous: there was no appearance indicative of its having a serous membrane.

8. The skin, contracted very much; the surface of the cutis vera became rough, as if glandular; the cuticle was rendered weak, almost pulverulent, scarcely offering any resistance.

9. The transparent cornea became corrugated and apparently opaque.

10. Stomach and intestines; their lining membrane was very much contracted and corrugated, and so altered in appearance, as not to be distinguishable; it was rendered also very weak, so as to be easily rubbed off. The same changes occurred in the peritoneal and muscular coat in a less degree. The glandular structure, especially of the duodenum, appeared more distinct.

Department, at Fort Pitt, made by Mr. Gulliver, clearly demonstrative of the existence of such a membrane, extended from the synovial fringes over the cartilage in question. In an instance of partial absorption of the cartilage of the patella, producing a small cavity, I found it covered with a delicate membrane, the normal one, I believe, unaltered.

11. Aorta; its outer membrane contracted and was very much corrugated; its inner, [tried when detached,] very slightly, and its strength appeared to be very little diminished; the middle layer curled a very little, and uncurled when put into cold water,—and its strength was not apparently diminished.

12. A portion of vena cava became condensed and corrugated, curling, spirally, outward, from the greater contraction evidently of the outer fibro-cellular coat; the wrinkles appeared in the inner coat; there was no distinct loss of strength. A portion of the vena portæ, was very much corrugated; one portion of its inner coat was suddenly rendered gelatinous; another and contiguous portion, harsh and friable.

13. The thoracic duct contracted, and was very much shrivelled.

In a few instances I endeavoured to determine, by measurement, the degree of contraction. A portion of dura mater, after the action of boiling water, was found contracted about one-half its length and width, but increased in thickness; a portion of tendo achillis gave nearly the same results; a small fasciculus of fibres, from gastrocnemius externus muscle, contracted less in length, only about one-third. To try to determine whether the contraction, in certain directions, was connected with a general condensation of substance, I ascertained the specific gravity of some of the textures before and after the

scalding agency, subjecting them, on each occasion, as nearly as possible, to the same degree of pressure between folds of blotting paper before weighing them. The results obtained are shewn in the following table.

Texture Tried.	Specific Gravity before Scalding.	Specific Gravity after Scalding.
Spleen	1041	1060
Dura mater	1047*	1058
Pericardium	1059	1074
Lower portion of thoracic aorta	1065	1075
Ditto, just below the arch de- prived of its outer coat .	1070	1072

These results would seem to shew that condensation attends the contraction,—that the bulk of the part acted on is actually diminished. This is also indicated, and I apprehend very clearly, by the effect on certain parts,—especially on the penis,—which, perhaps from its structure, is better adapted than any other to display it. Under the influence of boiling water, this organ becomes hard, and tense, and rounded; and from the blood *pressed out*, evidently of diminished volume.

* This low specific gravity of dura mater, was probably the effect of disease; in the body from which it was taken fluid was found effused between the membranes.

Before passing to another part of the subject, I may mention a small number of trials which I made to endeavour to determine, at what temperature, the corrugating power of hot water commences. Dura mater was selected for the experiment. Water of the temperature of 140° , poured on it, had no apparent effect; at 150° , it was doubtful; at 155° , there was a perceptible corrugating effect; at 170° , it was considerable; and, at 212° , the effect was at its maximum.

I shall now describe the effects witnessed on different textures from the operation of long continued boiling. As a preliminary, the effects of this agency on the principal constituent parts of the blood, or those into which it is resolved, when abstracted, may be mentioned.

1. Some serum of blood was coagulated by immersion in boiling water. The coagulum was inclosed in a linen bag and kept in boiling water, over a slow fire, for twenty-four hours. The matter of the coagulum was not in the least softened; shewed no tendency to become gelatinous; it was firmer and more brittle than when first coagulated, breaking with a conchoidal fracture. Wiped with blotting paper to remove adhering moisture, it was found to be of specific gravity 1031. This low specific gravity, perhaps, might have been expected, as when the serum was first coagulated by heat, there did not appear to be any condensation; the vessel which was filled with the serum, was equally filled with the co-

agulum; nor from long boiling was there any very distinct appearance of contraction.

2. A portion of fibrin of blood, the colouring matter and the serum separated by washing, was plunged into boiling water; it immediately contracted, and apparently very much. The water was kept boiling for 24 hours. The fibrin was not rendered gelatinous; nor did it appear diaphanous in the slightest degree. It was not entirely deprived of its elasticity; it was perhaps a little softer; but not distinctly so. Pressed between folds of blotting paper, it was found to be of specific gravity 1067.*

3. A mixture of red particles and serum, after

* In another trial, made on a fibrinous mass from the right ventricle of the heart, boiled for about twelve hours, its specific gravity was found to be 1049.

Considering the effect, above-described, of a boiling temperature many hours continued on the fibrinous part of the blood, it is very curious that coction or digestion of fibrin at a comparatively low heat, viz. about 99°, should afford a very different result: thus, if the blood-heat coction be continued for 48 hours, without stop, the fibrin is reduced to a semi-fluid state, coarsely resembling pus, and very closely resembling the puriloid matter into which fibrinous concretions formed in the heart, or blood-vessels, during life, are in part converted under peculiar circumstances. To Mr. Gulliver I apprehend belongs the merit of having first observed this peculiar effect of coction at a blood-heat on fibrin, and the analogy between its product and the result of the softening process on fibrin in the living body.* The analogy, however, it is right to observe, is not altogether perfect. In Mr. Gulliver's experiments on the coction of fibrin, I believe, without any exception, marks of putrefaction were developed in the fibrinous mass, which may conduce to the disintegration of the fibrin, and its reduction to a poultaceous state: farther on, it

* London Medical Gazette, vol. i. New series, p. 874.

the separation of the fibrin, by being pressed through linen cloth, poured into a vial, was immersed in boiling water ; the cruor immediately coagulated pretty firmly. The water was kept boiling for 24 hours. The coagulated cruor was rendered softer and more friable than the coagulum of serum ; its colour was a dull brick red, which it had after the first impression of the heat. Pressed between folds of blotting paper, it was found of specific gravity 1090.

In the following trials, the operation of boiling was carried on uninterruptedly for ten hours. The parts subjected to it were immersed in the water already in ebullition. They were all from a body 21 years of age, at the time of death. It may suffice to state briefly the principal appearances observed in each texture, or in each compound organ, at the end of the operation.

1. Cerebrum.—Both medullary and cineritious matter, seemed rather contracted, was rather harder and more friable, and felt greasy.

will be seen how rapidly, at a temperature between 80° and 90°, fibrin partially liquefies in undergoing putrefaction ; but in the fibrinous concretions, when undergoing softening, I have never observed any well-marked signs of incipient putrefaction ; the softened matter has never, that I have witnessed, emitted an offensive smell, and when it has been mixed with lime or caustic potash, it has yielded a very slight odour only of the volatile alkali. The softening of the fibrin, in the living vessels, *centrically*, [as is invariably the case as far as my experience extends,] whilst the surface of the mass is hardened so as to assume the appearance of a sac or inclosing membrane, does not seem to admit of explanation on any known principle : it is an important problem in its pathological relations and deserving of minute inquiry.

2. Dura mater, was thickened, rendered more transparent and elastic ; it was easily stretched and broken. Its outer surface was shrivelled. It admitted of being easily divided into two laminae ; the exterior of which was thinnest. The direction of the fibres was horizontal. No appearance of arachnoid membrane, or of a membrane reflected over its inner surface was perceptible.

3. The external investing membrane of the heart was rendered soft and imperfectly gelatinous.

4. The lining membrane of the cavities of the heart was rendered slightly gelatinous.

5. The cordæ tendineæ were rendered soft, not in the slightest degree viscid, but very friable ; they were pellucid and like cold jelly.

6. The valves appeared contracted and corrugated ; they were very friable, and slightly gelatinous ; their base was more so than their free margin.

7. The muscular substance of the heart was rendered very friable and soft. It showed a disposition to come away in spiral masses, when pulled asunder.

8. The outer coat of the pulmonary artery and of the aorta was rendered softer, and somewhat gelatinous, and slightly viscid, so as to be slightly ropy when drawn out. It could be traced down to the valves ; shewing, as it appears to me, and as is confirmed by the effect of maceration, that the semilunar valves of both great arteries are connected in part with this outer coat.

8. The middle coat of both vessels was rendered

more friable, but not more transparent, and not in the least gelatinous :* it was much less weakened and altered than muscle.

9. The inner coat too was rendered more friable, more difficult to separate as a continuous membrane, indeed it could be separated only in fine shreds, and it did not appear at all gelatinous.

10. The pleura was rendered soft, rather gelatinous and a little viscid.

11. The substance of the lung had become firm, compact, very friable, and of a greasy feel.

12. The liver, kidney, and pancreas, were rendered very similar; and their investing membrane had become very like the pleura.

13. The inner coat of the stomach and intestines was very much corrugated; it had not become in the least gelatinous, but very friable and easily separable from the cellular coat by rubbing.

14. The cellular coat, between the inner and muscular, was rendered very soft, somewhat gelatinous and viscid: it felt granular, as if it contained mucous glands.

15. The muscular coat had become very friable; not at all gelatinous; the muscular fibres were very distinct

* I have found the effect of long-continued boiling on the substance of the ligamentum nuchæ of the ox similar, and also similar on the thick, very elastic middle coat of the aorta, as in the arch, where this structure is most strongly marked in the ox.

16. The peritoneal coat generally was rendered soft ; in some places it was rather gelatinous and slightly viscid, in others friable.

17. The inner coat of the urinary bladder and of the upper part of the urethra, had become slightly gelatinous and diaphanous.

18. The muscular coat of the former was delicately displayed ; its fibres appeared very distinct ; they were very friable.

19. The cuticle of the tongue was rendered soft and very friable, and was very easily detached. It was thick above and spongy ; extremely thin below, and over the frænum indistinct. I attempted to trace it in continuation. Over the cryptæ of the amygdalæ I could not detect it ; nor in a satisfactory manner in the pharynx, in the immediate vicinity of the amygdalæ. The cryptæ and contiguous lining membrane had become gelatinous. In the œsophagus, the cuticle was very manifest, easily separable, indeed scarcely adherent, and very friable.

20. The cutis of the under surface of the tongue, extremely thin, was rendered slightly gelatinous and semi-transparent. The thick cutis of upper surface had become rather friable, especially the papillæ, as if composed chiefly of vessels. When it was removed, gelatinous projections were seen rising from the muscular coat corresponding to the papillæ.

21. The mucous coat of œsophagus had become rather friable ; some gelatinous matter appeared to

be diffused through it. The sub-mucous, cellular, and the muscular coats were affected much in the same manner as those of the stomach and intestines.

22. No cuticle or epithelium was discoverable in the larynx, or in any part of the *aspera arteria*. It was very carefully sought after, on the supposition, that it might exist of a very delicate nature.

The lining membrane of the larynx was found to be semi-transparent, elastic and gelatinous; the *cordæ vocales* were similar, as also the connecting membrane of the cartilages of the trachea.

23. The cartilages of the larynx had become much swollen and broken; portions of them were projecting out. They were very soft, nearly transparent, and quite gelatinous. The change in the epiglottis was less strongly marked than in the firmer cartilages; it was less uniformly swollen, soft, and transparent. The perichondrium generally was rendered gelatinous.

24. The cartilage of patella was found soft and semi-transparent, like stiff jelly.

25. The lining membrane of the capsule had become soft, rather gelatinous, and slightly adhesive.

26. The cellular membrane, surrounding the bursa, between the patella and the skin, was found soft, gelatinous, and ropy, admitting of being drawn out into threads possessing some elasticity.

27. The cutis over the patella was much corrugated; it had become soft, friable, and a little gelatinous; one surface was more gelatinous, the other

more friable. The outer surface was rough, from little protuberances of a gelatinous kind.

28. The skin of the scrotum appeared somewhat similar, but without the insulated little masses of jelly on its surface.

29. Its cellular membrane had become soft, gelatinous, and ropy. The tunica vaginalis was very similar; it was equably gelatinous and soft.

30. The albuginea was rendered soft and friable, not viscid. In appearance, it approached somewhat to the cartilage of patella, but was less transparent.

31. The testicle was found firm and very friable.

32. The conjunctiva of eye had become rather gelatinous, soft, and adhesive.

33. The cornea was rendered translucent, and friable, like the albuginea.

34. The sclerotic coat was similar: the cornea and this coat were not separable.*

35. The lens had become opaque white, firm and very friable,—not in the slightest degree gelatinous: it broke up into concentric laminae.

* An eye, an inch in diameter, measured from the entrance of the optic nerve to the cornea, and transversely, immersed in boiling water about ten minutes, was found contracted to a diameter of $\frac{1}{10}$ th of an inch; and the cornea from $\frac{3}{16}$ th to $\frac{1}{8}$ th; the latter was not corrugated or diminished in transparency. The boiling was then continued about ten hours; the globe was still more contracted, but the sclerotic having burst, it was not easy to measure it; the cornea was reduced to $\frac{1}{10}$ th of an inch. It and the sclerotic had become soft, not gelatinous; the one retained a considerable degree of transparency, and the other had acquired it.

36.* The articular cartilages investing the ends of the bones of the foot were rendered semi-transparent; soft, like stiff jelly.

37. The tendons of the foot had become similar; the tendo achillis was also gelatinous, but less completely so; there were whitish portions observable in it, as if from minute vessels.

38. The pancreatic duct was found changed in a manner resembling tendon; it had become uniformly transparent and gelatinous.

39. The outer coat of the ductus communis choledochus, was rendered very like the corresponding coat of the aorta; its inner coat was less altered; it had become diaphanous, slightly elastic, and imperfectly gelatinous.

40. The cutis of body of penis had become soft, gelatinous, and elastic.

41. The tendinous sheath of penis was affected very like dura mater; but was rendered softer; it admitted of separation in layers.

42. The corpus cavernosum and spongiosum had become very friable, slightly elastic, and only slightly diaphanous. No epithelium could be detected, continued from the cuticle of the gland into the

* This and the following trials were made on parts of a limb, amputated on account of extensive disease of the knee joint, following a fall; the patient was 21 years of age. The amputated limb was boiled eight hours. It was curious to see the general disruptive effect. All the bones of the feet were dislocated; the tendons broken; the cuticle almost entirely detached. The toes and metatarsal bones were bent downwards.

urethra. The lining membrane of the urethra was rendered gelatinous.

43. The glans penis throughout had become soft, elastic and gelatinous. Its cutis was very gelatinous; minute papillæ, semi-transparent points, appeared regularly distributed over it.

In many instances, the specific gravity of the parts was ascertained, after the operation of long continued boiling. The following Table exhibits the results obtained. The number in the Table, corresponds with the preceding numbers.

Table shewing the specific gravity of different textures after long boiling.

No.	Description of Texture.	Sp. Grav.
2	Dura mater	1023
7	Columna carnea of left ventricle of heart	1052
8	A section of aorta (including all its coats) close to the coronary arteries	1061
12	Liver	1046
—	Lung	1061
—	Kidney	1071
—	Spleen	1070
19	Muscular substance of tongue	1062
23	Thyroid cartilage	1012
31	Substance of testis	1070
36	Cartilage of os calcis	1020
37	Tendo Achillis, close to its termination	1034
A	Cutis of heel*	1029
B	Cuticle from heel	1076
	Dura mater	1030
	Abdominal vena cava	1040
	Its outer coat detached	1034
	Thoracic aorta just below the arch	1068
	Duodenum close to common biliary duct	1050
39	Common biliary duct, with a portion of pancreatic duct attached	1038
40	Cutis of dorsum penis	1033
41	Tendinous sheath of dorsum penis	1044
43	Glans penis	1045

* These parts, to which a letter is prefixed were portions of the

All these results, received even favourably, can be considered only as approximations. They would be more valuable for the purpose of comparison, had the specific gravity of each part been ascertained before being subjected to boiling. However, imperfect even as they are, I hope they may not be without use as data for reasoning on, in forming general views of the analogies of textures.

The results generally are not so accordant with those of Bichat, as described by him in his *Anatomie Generale*, as might be expected: nor are they in my opinion favourable to his views of distinct differences in the qualities of certain membranes, which characterise his theory. He states that cellular membrane, and fibrous membrane are very differently affected by coction. In my trials it did not so appear. Judging merely from the effect of coction, the conclusion, which seems most legitimate is, that which harmonises with the views of Haller,—that the majority of the white membranous and fibrous parts, are of analogous composition, differing rather in degree than kind; and not essentially different from cellular tissue; and each differing in like manner in degree in different situations, so as not to allow of any individual membrane or continuity of surface throughout its course being considered identical.

amputated limb: those without the prefix of a letter or number, were taken from a body aged about 21; the latter were boiled eight hours.

Farther on I shall have occasion to recur to this subject.

When engaged in this inquiry, I had intended to investigate more in detail the effects of coction, and I commenced making observations on the comparative tendency to putrefaction of the textures subjected to the boiling process. From the partial trials then made, it appeared, that all those parts which were much softened and gelatinized by boiling, had become by the change much disposed to putrefy. It was remarkably the case in the instance of tendon, cartilage, and fibrous membrane. After being boiled, they underwent the putrid liquefaction even more readily than muscle: the effects in these instances, as in muscle, liver, lung, spleen, &c. in which, at least in the majority, there appeared to be an augmentation of specific gravity, rather than a diminution, was less clear: and I am doubtful, whether, the coction really increased or diminished the putrefactive tendency.

A few experiments about the same time were made, on the fitness of boiling, as an aid in the preservation of anatomical preparations. The result was favourable, shewing that those parts, which contain much blood, and which are themselves but little altered by a short application of the boiling temperature, may immediately be put into spirit of wine, and kept without its being necessary to change it. Thus, in February 1829, a portion of blood coagulated by heat,—a portion of liver and spleen, merely in-

durated by immersion in boiling water, were placed in proof spirit. In May of the following year, they were found in excellent preservation, the spirit clear, without sediment, and only very slightly coloured.

XV.

ON THE AGENCY OF ATMOSPHERIC AIR ON DEAD ANIMAL MATTER; AND ON THE PUTREFACTIVE PROCESS.

AMONGST many questions of interest involved in the subjects enumerated above, there are two which stand forward in a prominent manner; viz., whether dead animal matter before putrefaction commences has the power of absorbing air; and whether the putrefactive change, is attended with elevation of temperature. Bearing on these points, although not exclusively so, I shall relate three sets of experiments which I have made: one, on the agitation of fresh animal matter in atmospheric air; another, on its exposure to this mixed air, confined without agitation; and a third, on exposure to the atmosphere, under circumstances favourable to putrefaction: and, I shall conclude with some general remarks on the putrefactive process.

My attention was first specially directed to the inquiry in Malta, in 1829. Of the results then obtained and published in the Edinburgh Medical and

Surgical Journal, I shall now give only such as, on revision, I believe to be correct and liable to no serious objection.

1.—*On the Agitation of dead Animal Matter in Atmospheric Air.*

The results of the latest experiments which I have made on the agitation of blood and serum in air, have already been stated, shewing, that, independent of putrefaction, both venous and arterial blood have the power in question; but that serum does not appear to possess it, in an appreciable degree. Respecting serum, I think it right to express myself in this qualified manner in consideration of the well established fact,—first I believe pointed out by Dr. Priestley,* that venous crassamentum, when guarded from the atmosphere merely by a thin covering of serum acquires superficially the arterial hue; indicating the passage, if not the absorption of oxygen gas.

To try the effect of the solids, — two substances have been selected as best fitted to give decisive results, viz., cerebral matter and muscular fibre. The experiments on the agitation of these substances have been made in a manner very similar to those described on blood and serum. The brain of a sheep was used—its masseter muscles and

* Experiments and Observations on different kinds of Air; by Joseph Priestley, Birmingham, 1790, vol. iii. pp. 365. 370.

tongue. As soon as the animal was killed in the usual way, its head was removed and brought from the slaughter house. The brain was extracted whilst still warm, and immediately immersed in water, just in sufficient quantity to cover it: it was cooled rapidly by placing the vessel containing it in running water; and quickly broken up and reduced to the state of an emulsion. In this state, with as little water as possible, it was poured into the double-mouthed bottle of the capacity of 32 cubic inches, provided with stop cocks and a bent tube, as already mentioned, and well shaken in atmospheric air for about two minutes. On turning the stopcock, with the bent tube attached connecting it with water, the water did not rise in the tube,—on the contrary, a few bubbles of air escaped. The result obtained using rather more than a quarter of a pound of the masseter muscles, cut into very small pieces under water, was similar; as it was also, employing the whole of the tongue divided in the same manner. The cerebral emulsion, and also the first-mentioned muscle was allowed to remain in contact with the air in the bottle for nearly twenty-four hours, without further agitation; after which, on turning the stopcock, there was a slight rise of the water in the tube; but, I apprehend, not more than might be fairly attributed to a change of temperature which had taken place in the air of the room in the interval; a fall, namely, from 55° to 51° . What was the kind of air disengaged, I did not attempt to ascer-

tain. It might have been common air, expelled, owing to expansion from perhaps a little heat produced by the friction of agitation. Be this as it may, the quantity expelled was so very small, that the determining of its nature, would have been attended with extreme difficulty.

As regards the conclusion to be drawn from these experiments, the same I apprehend may be admitted as in the instance of serum; viz., that neither muscle nor the cerebral substance perfectly fresh, have the power of absorbing oxygen in an appreciable quantity; and judging analogically, it may be inferred, at least as highly probable, that the other textures less prone to change, are also destitute of this power.

2.—*On the Exposure of dead Animal Matter to Atmospheric Air, at rest in Confinement.*

As bearing on the subject of putrefaction, as well as in part on the last, in relation to the animal textures,—I shall give the particulars of many experiments made at Malta during the summer season, on the exposure of dead animal matter to measured quantities of atmospheric air, confined in jars over mercury at a temperature between 80° and 90° , a heat at which animal matter, as is well known, is peculiarly liable to change and become putrid. I shall first notice the results obtained, using blood and its principal constituent parts, physiologically considered.

The blood subjected to experiment was venous blood. It was placed in a receiver over mercury in a measured quantity of common air, a few minutes after it had been drawn, having been quickly cooled by immersion in water. During the first twelve hours, the volume of air over it was not sensibly changed. Soon after there was a diminution of the bulk of the air, and this diminution gradually increased. When greatest, it was nearly equal to the volume of the blood. This was accompanied by a change of form of the crassamentum; it, as it were, dissolved away, and an apparently homogeneous fluid was formed. And next there was an augmentation of the bulk of the air. By examination at different times, I found, that the volume of air remained unchanged until putrefaction commenced;* that carbonic acid was formed and absorbed by the blood; that the diminution of the volume of air continued till the blood was saturated with carbonic acid; and that its augmentation was owing to further evolution of this gas, connected with the formation of ammonia and the progressive changes of the putrefactive decomposition.†

* This may appear contrary to what has been already stated relative to the absorption of oxygen by blood previous to putrefaction: but is not in reality; without agitation, the putrefaction of blood, I believe, commences [at least at a high temperature,] before the absorption of air by it, is demonstrable.

† The products of the putrefactive decomposition are, I believe, very much modified by temperature; so that were the results obtained in summer and winter compared, they would be found greatly

The air over serum of blood confined over mercury in the same manner, remained many days without undergoing any change of volume; and so long, the serum retained its transparency. At length a turbidness appeared in it,—putrefaction had commenced, and the volume of air diminished, owing to the absorption of carbonic acid gas which had formed. When the serum was saturated with carbonic acid gas,—the putrefactive process continuing, the volume of air slowly increased.

Results very similar were obtained, with the colouring matter alone; or (which is more correct) the mixture of serum and red particles, obtained by pressing the crassamentum gently in linen. The changes in this instance were much the same as in the preceding; they took place more slowly than when blood with all its ingredients was employed, and less slowly than when the serum of blood was used.

The changes exhibited by the fibrin of the blood were the most rapid of all. In less than twelve hours, it lost its firmness; a small diminution of the volume of air had taken place; a little carbonic acid gas was found in the residual air, and oxygen had disappeared about equal in volume to the loss

to differ. At a low temperature, as between 40° and 45° , in many instances, I have found both the blood of man, of the sheep, and of the ox, to decompose without evolving any air: there was a continued absorption of oxygen, and the formation of a large quantity of carbonate of ammonia.

by absorption, together with the acid gas generated. In twenty-four hours or little more, the fibrin had become semifluid or poultaceous; the oxygen of the air had disappeared entirely, or nearly so, and its place was supplied by carbonic acid gas, excepting a diminution (which it may be inferred was carbonic acid gas absorbed) very nearly equal to the volume of the semifluid formed. As the putrefactive process proceeded, the fibrin divided into two parts—one poultaceous and a little frothy, occupying the spot where the fibrin was first put,—the other liquid, surrounding the poultaceous matter; and the volume of air gradually increased, carbonic acid gas being evolved, attended with the formation of ammonia and the other changes belonging to advanced putrefaction.

I shall next mention the results obtained in similar trials on different textures taken from animals just killed, as the rabbit and ox,—or from the human subject a few hours after death. Paying as much attention as possible to uniformity of temperature, and watching the experiments carefully, I have never been able to observe any immediate absorption of air. In several instances, as of muscle, liver, intestine, &c., there was no diminution but a progressive increase of volume from the evolution of air. In the few instances in which a diminution of volume did take place, it was very inconsiderable; and as this was accompanied by incipient putrefaction, and generally attended with the appearance of some car-

bonic acid gas, is there not good reason to infer, that it was entirely owing to the absorption of carbonic acid gas formed, especially as the moisture of the part or the fluid which it contained was sufficient to account for it, and the oxygen which disappeared was equal to the absorption added to the carbonic acid gas remaining? In corroboration of this view, I may mention the difference of results obtained, when animal substances were exposed to putrefaction under increased, and under diminished pressure by means of a column of mercury acting on the air, in which the substances were confined,—in one instance, in the closed end of a tube bent upwards; in the other, in the closed end of a straight tube, both of course placed perpendicularly. When the pressure had been increased by the weight of eight inches of mercury, and diminished by a counterpoise of sixteen inches, the results were well marked, especially when using very putrescible substances, as the fibrin of the blood and muscular fibre. With the increased pressure, at a temperature of about 76° , fibrin being used, in twenty-four hours there was a small diminution of volume of the air, with just sensible marks of putrefaction having begun; whilst with the diminished pressure, there was a small increase of volume and a manifest appearance of putrefaction having commenced. Now, it follows of course, that the carbonic acid gas first formed, would be more powerfully absorbed, and retained with the increased than with the diminished

pressure; but if oxygen was absorbed chemically, there is no reason to suppose it would produce any difference of effect except in degree.

The experiments, of which I have just related the results in part, were, as already mentioned, all made at a comparatively high atmospheric temperature,—favourable to rapid putrefaction. The degree of rapidity of change, I may remark, varied according to the nature of the texture. Brain, muscle, spleen, liver, and the substance of the glands generally, produced the most rapid change. In a few hours carbonic acid gas might be detected in the air exposed to them; and generally in less than twenty-four hours there was air generated, and an increase in the volume of the air. The soft white parts were next in degree of rapidity,—such as the cutis, periosteum, dura mater, and I may add, stomach, intestinal canal, vein, artery, and adipose structure. Over these, in about twenty-four hours, carbonic acid gas could generally be detected in the air; and generally, in about forty-eight or seventy-two hours, a little air was generated. Tendon, intervertebral substance, cartilage, cuticle and bone, were slowest in progress. In two or three days, carbonic acid appeared in the air in which they were contained; but many days elapsed, before there was any increase of volume indicating the generation of gas, excepting the parts belonging to a young animal. After the putrefactive process had commenced, it proceeded, although the oxygen of

the atmospheric air was entirely consumed: carbonic acid gas continued to be disengaged, in which sometimes a little carburetted hydrogen might be detected, and sometimes a little sulphuretted hydrogen;—ammonia was constantly formed; but I have not been able to satisfy myself that any azote was either absorbed, or evolved, at least in the early period. The appearance of carburetted hydrogen was most striking in the instance of muscle; it occurred irregularly; sometimes early in the process, more frequently at a later period; one day, it might be detected in the air generated, not in the next. A softening of the parts, like a deliquescence, always marked the progress of the change, and most of them became poultaceous. After about a fortnight or three weeks, brain, muscular fibre, the parenchymatous substance of the glandular structures and the spleen, were reduced to a very soft state, and on washing them, the soft matter, the result of putrefaction, was removed, and the vascular structure of the part well displayed; and in the instance of muscle, the tendonous attachments also, and the aponeuroses: and these in their turn yielded and became decomposed, the process being allowed to continue, as I shall have a better opportunity in describing in the next section when noticing the same changes taking place in the open air.

3.—*On the Exposure of Dead Animal Matter to the Atmosphere, under circumstances favourable to Putrefaction,—with a view to the question, Is the Putrefaction of Animal Matter attended with an Elevation of Temperature?*

I shall give in some detail, the experiments which I have made to endeavour to determine this question. It may be premised, that they were all made at the same time and place as the preceding; that the subjects of them, as soon as possible, after they were taken from the body, were put into glass vessels, lightly covered with, and muffled in dry flax, for the purpose of diminishing the influence of the atmospheric air, as a cooling medium; and that a vessel holding water, was placed contiguously, for the sake of comparison in relation to temperature. I shall commence with the blood and its different ingredients or parts into which it may be separated out of the body; and from them proceed to the solid textures.

Blood.—On the 19th August, about a pound and a half of venous blood, taken from a young man, labouring under a slight febrile ailment, moderately firmly coagulated, having been abstracted about three hours, was broken up, so that the colouring matter, the fibrin and serum were well mixed, and immediately put into a glass vessel in the manner premised for observation. On the 20th at noon, its temperature did not differ from that of the water

contiguous. At 6 P.M. of the same day, when the temperature of the water was 78° , that of the blood was 80° , and its odour was peculiar and rather offensive. On the 21st, when the water was 79° , the blood was the same as the day preceding. On the 22d, when the water was 80° , the blood at the surface was 82° , and a little below the surface 81° . Its smell was offensive and ammoniacal, and its liquidity was increased. On the 23d, the temperature of the blood was the same as that of the water; and so it continued, whilst slowly undergoing decomposition.

The colouring matter of blood.—About two pounds of the colouring matter of the blood of an ox, from which the fibrin had been separated in the usual way by agitation, but not deprived of serum, was exposed to observation on the 19th of August. It was examined once or twice daily. Its temperature never rose perceptibly above that of the water. On the 22d, it had an offensive smell, and mixed with lime, gave off an ammoniacal odour. In a few days the odour became more strongly ammoniacal and less offensive; and so it continued afterwards, whilst gradually undergoing the changes peculiar to its very slow decomposition.

Serum of blood.—The observations which I have made on the putrefactive decomposition of this fluid, do not indicate any sensible elevation of temperature attending it; a result, which might be expected, considering that its putrid changes take place very

slowly, and that many months are required for its complete decomposition, whether the quantity of serum be large or small, and even when freely exposed to the air in a damp atmosphere.

Fibrin of blood.—On the 19th August, at 4 P. M. the fibrin of the whole blood of an ox was separated in the usual manner, by agitation, and was immediately well-washed and drawn out loosely, so that its surface might be well exposed to the air. In its moist state, as it was placed for observation, it weighed about four ounces and a quarter. On the following day, when the water was 78° , the fibrin about an inch below the surface was 85° ; its odour was peculiar, but not fetid; this was about noon. At 2 P. M. the water being of the same temperature, the fibrin was 90° ; its odour was peculiar and rather offensive. At 6 P. M. the water the same, the fibrin had fallen to 87° . It had become soft, as it were melting or deliquescing, and in consequence had subsided so much, that it occupied only about half the space it did when first introduced, and of course was much less exposed to the action of the air. It was slightly alkaline, and a little of it, mixed with hydrate of lime, emitted a distinct ammoniacal odour. On the 21st, the water being 79° , the fibrin was only 80° . It was semifluid, of a light brown colour, and had a peculiar, rather disagreeable smell, distinctly ammoniacal. On the 23d, its temperature and that of the water were the same; it was of a light grey colour, so fluid as to admit of being poured out in

drops, and it had a pretty strong and tolerably pure ammoniacal odour. After this, there was no sensible elevation of temperature attending the changes which slowly took place during its progressive decomposition.

These experiments on the blood and its ingredients, I may remark, are not solitary ones; they have been repeated several times; and the results have been similar; and similar generally, so far as I have observed, whether the subject was the blood of the ox or human blood.

Muscle.—On the 25th August, the psoas muscle of a man who died the same day, and of whose case I shall take brief notice in an Appendix, as No. 1, was placed for observation. No change of temperature was observable till the 27th; then it was 2° higher than the water; its odour was not putrid. On the following day, its temperature was the same; its odour peculiar and disagreeable, but not distinctly fetid; its surface was sprinkled with the ova of flies, and with minute larvæ. On the 29th, it was 3° above the water. On the 30th, at the surface, it was 5° higher, whilst about an inch below, its temperature was the same as that of the water. On its surface there was some froth, and larvæ in great abundance. The odour of the surface was peculiar and ammoniacal; of the part below the surface, peculiar, but not distinctly ammoniacal. On the 31st, it was 7° above the water, when larvæ abounded, and were feeding voraciously, and there was a strong

ammoniacal odour. The experiment was then interrupted by excluding atmospheric air; the froth subsided, the larvæ died, and the temperature of the part fell to that of the water. A similar experiment was made with the ventricular portion of the heart of the same subject, and with precisely similar results. Other experiments have been made in a somewhat different form, and results have been obtained also somewhat different. When flies have been excluded, muscle has undergone a comparatively slow change; it has gradually softened, become unctuous, acquired a peculiar odour, lost its cohesion, and has become converted into a reddish semifluid, not unlike chyme, with a very strong ammoniacal odour. In one instance, this change took place between the 6th and the 21st of August. The temperature was not elevated more than 2° at its maximum. When the muscular fibre was unravelled, and an extensive surface exposed to the air, the change was very much more rapid; the conversion of muscle into poultaceous fluid took place in six days, and the temperature was elevated 7° .

Brain.—I shall give two different experiments made on this organ entire, or nearly so, deprived of its membranes, belonging to bodies No. 1, and 2. That on No. 2, was commenced on the 14th July. On the 15th, its temperature, and that of the water were the same, viz. 76° . On the 16th, at its surface, it had risen 3° ; whilst the deeply seated portions varied according to their depth, from 2° to 1° .

The surface was then soft, brownish and fetid; what was below the surface was apparently little altered. The temperature of the brain continued increasing till the 21st, when it had risen 11° , then its surface was covered with froth; larvæ of large size were in great plenty, and were feeding greedily; and the odour was strongly ammoniacal. From this time the temperature gradually fell. On the 31st, it was only 2° higher than the water; on the 1st August only 1° ; and on the 4th, there was no difference. At this time, its surface was grey, soft, of saponaceous appearance, free from larvæ, and emitting a powerful ammoniacal odour; and even then, two inches below the surface, it appeared to be but little changed. The experiments with brain No. 1, was begun on the 25th July. The highest temperature attained by it was only 1° above that of the water; and on the 11th August, it was the same as that of the water. In this instance, the number of larvæ generated on its surface was less than in the former; their growth was less rapid; they fed with less avidity,—the surface was comparatively tranquil, and the ammoniacal odour comparatively faint; and latterly a rancid peculiar odour was rather predominant.

Lung.—It belonged to body No. 1, and was put into the vessel for observation, divided into portions not very small. The highest temperature which it acquired was on the 30th July, then the water being 77° ; its temperature, at the surface, and just below

the surface, varied from 78° to 90° . Where the low temperature was observable, there it was tranquil and without larvæ; where the temperature was high, there larvæ abounded and were in great activity,—the space they occupied was surrounded by a margin of froth, the ammoniacal odour was strong, and the portion was becoming of a blackish hue and liquid. The experiment was then interrupted by the exclusion of air. On the following day the larvæ appeared dead, and the temperature had fallen to that of the water. On the 2d August, atmospheric air was re-admitted. The temperature of the whole, never again exceeded that of the water perceptibly, though it did partially, as in portions where larvæ were collected feeding, and where froth was produced: thus, on the 6th August, whilst the surface in general was covered with a greyish byssus, and was only at the temperature of the water, in one spot about an inch in diameter it was 6° higher, and there in the midst of froth, there was a brood of larvæ rapidly growing and eagerly feeding. On the 27th August, the whole presented the appearance of an inky fluid, of the temperature of the water, and strongly ammoniacal, apparently deriving its black colour from carbonaceous matter liberated and suspended, mixed with particles of cellular tissue, and at the bottom with shreds of pleura and fragments of vessels, which became apparent on washing.

Liver.—It belonged to body No. 3, and was

placed for observation on the 3d August. On the 5th, its temperature was the same as that of the water; its odour was offensive, but not ammoniacal; a good deal of reddish fluid had exuded, and a little air was in the act of disengagement. On the 4th, in some places it was 1° higher, and in others 2° . Where highest, some froth was produced, and many small larvæ were feeding. The temperature gradually increased till the 10th, when it was raised 12° ; its surface was covered with larvæ feeding voraciously in froth, and nearly full grown, and the odour was powerfully ammoniacal. From this time, it gradually decreased, till on the 13th, it did not exceed that of the water; most of the larvæ were dead; the odour was very ammoniacal; the surface was free from froth, and of a dark brown colour, in some places almost black and of a soft poultaceous consistence. Beneath the surface the change decreased with the depth; and in the lower part, the liver was comparatively little altered; its parenchymatous substance was softened, of a grey hue, and easily removed by gentle pressure, aided by a stream of water; whilst the vascular structure, (at least all but the very minute branches) retained sufficient strength to resist the pressure and water, and accordingly became well displayed by the operation.

Intestines.—These were from body No. 4, and were placed for observation, after having been washed out, on the 3d of July. Their temperature on the 4th was the same as that of the water, though the odour

arising from them was offensive. On the 5th their temperature was 2° higher; on the 6th the same as that of the water; and so it continued, although a great deal of fetid air was disengaged, and the surface had become poultaceous and emitted an ammoniacal odour. The stomach of No. 1 afforded results very similar. Its temperature never exceeded that of the water more than 1° , and that only partially, where a few larvæ were feeding, and a little froth was excited. And, in both instances, below the surface two or three inches, there was little change. After a month, the parts appeared entire; but, on minute examination, after having been well washed, the tissue which remained seemed to be chiefly the submucous cellular and the peritoneal; neither the inner coat, nor the muscular, could be recognized; and even the white tissues were, in part, losing their cohesion, and exhibited evident marks of slow decomposition being in progress.

Cutis of inside of thigh, dura mater, tendo Achillis.—These parts were taken from body No. 1, and were placed together in the same vessel for observation. They underwent change slowly; their temperature gradually rose till it was 3° above that of the water, viz.,—on the 31st July, when the odour from them was slightly ammoniacal, their surface unctuous, and there were a few larvæ feeding. After the 2d August, the difference of temperature was 1° , and it was the same on the 15th of that month. A good deal of yellowish poultaceous

matter was formed, by which the parts were hid. It was washed off, and the different textures were found nearly equally changed; their cohesion irregularly diminished; the fine connecting tissue, or most delicate fibre, dissolved entirely or in part; holes formed in the two first-mentioned textures, and the coarser fibres, or fasciculi of fibres, separated in the last. On the 1st September they were all reduced to an ammoniacal poultaceous state, in which only a few minute tendinous fibres were distinguishable.

Aorta.—A portion of this vessel, belonging to body No. 1, placed for observation, was 1° higher than the water on the 30th July. It had a peculiar and ammoniacal odour, and its surface was becoming unctuous, especially its compact fibrous sheath, or the third coat from the inner one. On the 11th August there was an elevation of 3° ; a good deal of light-brown poultaceous matter had formed; the odour was strongly ammoniacal, and many larvæ were feeding. On the 16th August, it was entirely converted into a semifluid opaque mass, of a light-brown colour, and strong ammoniacal odour, and its temperature was the same as that of the water. I have also made trial of the different coats of the aorta, placed apart for observation, viz.,—the loose outer cellular, containing some fatty matter,—the adjoining compact cellular, or rather fibrous,—the next commonly called the middle coat, and the inner coat,—taken from body No. 5. The changes in all of them proceeded much in the same manner as has been

described in the preceding instance, with the exception of the loose cellular coat, which, in that instance, was excluded; it did not keep progress with the rest,—after a month's exposure it was very little changed.

Intervertebral substance.—A portion from the lumbar spine was taken from body No. 5, on the 7th of August. On the 13th, it had a peculiar odour, neither putrid nor ammoniacal. After a month, it was very little changed; and, during that time, its temperature was not sensibly elevated above that of the water. From other experiments, I have ascertained, that a very long time is required for the decomposition of this substance, and that even after a year, it is not very considerably changed.

Besides the preceding, I have made trial of other parts, as the pancreas, kidney, spleen, prostate. They have undergone change rapidly like the liver; their temperature has been considerably elevated; they have all softened, as it were liquefied like fibrin, and ammonia in great quantity has been generated.

I apprehend, more than sufficient has been adduced, in the results of the preceding observations, to decide the question proposed—“Whether or no the putrefaction of animal matter is attended with elevation of temperature?” It seems proved, that those parts which undergo rapid change, as the fibrin of the blood, brain, muscular fibre, and the parenchymatous substance of the glandular structures, give off a good deal of heat in being converted from the solid into the liquid form: and, as other parts

undergo a similar change, and are ultimately reduced to a liquid poultaceous state, and, as in some of them, there is a small elevation of temperature, may it not be fairly inferred, that the apparently inferior degree of heat generated by them, is merely owing to slowness of change: and, accordingly, whatever accelerates the process of decomposition, seems to promote increase of temperature. Minute mechanical division has this effect in a remarkable manner, as was witnessed in the instance of the fibrin of the blood, and of muscular fibre; the effect is probably owing to the very extensive surface exposed to the action of atmospheric air. The presence of larvæ too, has the same effect very strikingly, and probably in the same way. They tend to separate the parts, to agitate the surface, and to introduce air beneath the surface. It is curious to watch the progress of these animals in their growth, and still more so in their operations. When their food is very nutritious, the almost microscopic ova, in forty-six hours are converted into large maggots. When they have nearly attained their full size, they feed with extraordinary voracity, as if aware that their lives depended upon their activity. The whole of a numerous brood might be observed, side by side erect, with one extremity in the ammoniacal poultaceous mass, pumping up nourishment, and with the other and narrower extremity in the atmosphere, the orifice of its canal dilated, seeming to pump down air; and in the act,

by the difference of the specific gravities of the two fluids,—it may be inferred that the perpendicular position of the larvæ is maintained, whilst probably by the action of one on the other, a compound is formed fit for assimilation and for forming a part of the new animal, the putrefactive process at the same time being accelerated. It appeared probable at first view, that the larvæ themselves might generate and possess a comparatively high temperature; but, on the whole, the observations which I made to endeavour to determine this point, are not in favour of the notion. When the larvæ are distended with putrid matter, exhaling ammonia and rapidly undergoing change, then their temperature, as might be expected, is always above that of the atmosphere; but, when they are free from putrid matter internally and externally, although collected together in a large number, in a small space, as in a phial, a delicate thermometer placed in the midst of them, did not indicate the generation of any sensible degree of heat. And, in conformity with this, I found that the larvæ by themselves vitiated the air very slowly; indeed, I believe that their dead bodies vitiate it more rapidly than their living. The changes in the dead bodies, in putrefying and becoming semi-fluid, accompanied by the formation of carbonic acid and ammonia, are more rapid than the changes in their living bodies in passing from the larvæ through the chrysalis state to that of the imago, or in becoming as it were solid, which change

seems to be accompanied chiefly by the exhalation of water, and the formation of a very little carbonic acid gas. Mixture probably is another circumstance which favours and accelerates the putrefactive process and the evolution of heat. The two circumstances already adduced may partly act in this manner, as well as by exposing a greater surface to the air. In the saccharine or vinous fermentation, the effect of mixture is well marked. So long as the sweet vegetable juice is retained in the tubes or cells of the plant or fruit, it resists change; but, the instant the tubes are crushed, and the cells broken down, the fermentation under favourable circumstances commences. I suspect something similar to this happens with animal matter. It is known to every one that bruised meat putrefies very rapidly. An egg, carefully put by, exposed freely to the atmosphere, without any covering over its shell, will remain free from putridity for months; and in a dry atmosphere, will lose the greater proportion of the water which it contains, and on breaking it, the yolk and white will be found dry and contracted. But, if the yolk and white are intimately mixed by violent agitation, then, I believe, the mixture, even in the shell, will soon become putrid. Certain I am that when freely exposed to the air, out of the shell, the yolk being merely immersed in the white, they do not putrefy, though, if the two are mixed, in a short time they undergo a change; they become highly putrid, and

the temperature of the mixture rises several degrees.

4.—*Some General Remarks on the Putrefactive Process.*

What is animal putrefaction, or the putrid fermentation? I am not aware that this question has yet been replied to, in a satisfactory manner, or that the present state of our information is such as to admit of making the reply free from many obscurities. The little information I have collected bearing on the subject, I shall bring forward without hesitation. Under diminished pressure, conducted in tubes over mercury, the putrid fermentation appears in some of its changes analogous to the vinous. The elements of vinous fermentation appear to be leaven, which remains unaltered,* and sugar which undergoes change; and the helping circumstances, a certain degree of dilution with water, and a certain temperature. The elements of the putrid fermentation are less distinct. If there be, as is probable, an animal leaven, it has not yet

* The same portion of leaven may be repeatedly used. I have preserved the leaven obtained from the grape for nearly twelve months, by pouring off the fermented fluid, as soon as the leaven had subsided, and adding fresh sugar and water. How leaven acts is one of the mysteries of chemistry, and there are others of a like kind,—as the influence of carbonate of lime in the formation of salt-petre,—of sulphuric acid in the formation of sugar from starch, &c.

been isolated. Whether the presence of oxygen is essential to the commencement of the vinous fermentation is matter of doubt. But the presence of oxygen seems to be undoubtedly necessary to the commencement of the putrid fermentation, and only to its commencement, as we have seen that it goes on over mercury, after atmospheric air has been entirely excluded.* The products of the vinous fermentation in the simplest form, appear to be very few. Alcohol is the only liquid product known,—and carbonic acid gas I believe is the only essential gaseous product. The disengagement of azote, probably, when it occurs, is accidental; may it not be referred in the early stage of the process to the atmospheric air entangled in the materials, and in the advanced stage to the decomposition of a little leaven? I have made many experiments on this subject, which have led me to this conclusion. The products of the putrid fermentation seem to be

* From experiments already related, it appears that venous blood as well as arterial, contains oxygen in a peculiar state of combination associated with the red particles: this oxygen may be sufficient to excite putrefaction after death, in every part of the body penetrated by the blood independent of the atmosphere; and to account for the very rapid manner in which bodies putrefy, if abounding in blood, as is often the case in instances of sudden death, and for their putrefying under water in instances of death from drowning. The very tympanitic state which bodies acquire so circumstanced,—indicates, that their different textures, especially the integuments, are little pervious to air,—which is also in accordance with the results of Priestley's observations on the power of bladder when thoroughly wet to confine air.

more numerous. Carbonic acid gas is the principal gaseous one. Sulphuretted hydrogen, and carburetted hydrogen, may be occasionally detected. Azote, I believe, is not disengaged, excepting at a very advanced period, when, probably, the products themselves are decomposing. The products not evolved in the gaseous state appear to be chiefly ammonia, kept down in confinement awhile by carbonic acid and water; and a substance, or matter in a semi-fluid form which has a considerable resemblance to chyme, and which appears to be the proper food of the larvæ of many different kinds of the extensive genus *Musca*,—some preferring the matter from muscle, some from tendon, &c. And this matter itself seems to undergo decomposition, and to be converted into ammonia, and a substance more resembling extractive matter after it has been exposed to the air, than any other with which I am acquainted, excepting one, procurable by the spontaneous decomposition of some kinds of vinegar to which atmospheric air is allowed to have access. During the decomposition of the vinegar, carbonic acid is formed by the union of carbon with oxygen. The vinegar gradually loses its acidity, and at the same time a pellicle forms on its surface, and gradually increasing, becomes a thick, strong, tough, transparent membrane,—elastic, strong, and transparent when dry, insoluble in water, and having great power of resisting change, whether acted on by heat, by acids, or alkalies. Very analogous to

this, is the substance alluded to, which I have witnessed on the surface of serum and fibrin, and of several solid parts in water in experiments on their putrefaction, continued from six to twelve months. The product from vinegar does not contain azote; that from the animal fluids and textures has afforded ammonia when decomposed with lime, at a red heat; but, as I am not sure, that any specimen of it, which I have yet examined has been perfectly pure, I cannot be certain that this difference of composition really exists between the two.

Under ordinary atmospheric pressure and exposure to the atmosphere, the results of the putrefactive fermentation are various and modified; the circumstances become complicated, and the difficulty of appreciating them is vastly increased. The gases generated and absorbed, in part seem to react, and to exert a powerful modifying influence, both as retarding putrefaction and giving rise to combinations which have a similar tendency; and inspissation at the surface from evaporation, by forming a crust, more or less impervious to the air generated beneath, as well as to the external air, has a similar effect. Carbonic acid gas is powerfully antiseptic,—so are ammonia, and carbonate of ammonia, and sulphuretted hydrogen, as I have ascertained by experiments made expressly for the purpose. Thus, blood saturated with carbonic acid gas, did not putrefy so soon, as another portion to which none of the acid gas was added. A solution

of the sesquicarbonate of ammonia, in the proportion of three grains to the ounce of water, prevented the putrefaction of a portion of muscle immersed in it about a month; and a solution of the strength of five grains to the ounce of water, preserved a piece of jejunum more than eight months, and I know not how much longer; and displayed the minute structure of the part, in a very striking manner. Reduction of temperature is another disturbing circumstance, which tends, I believe, to give rise to new combinations, especially to the formation of adipocire, and to render the problem as to the changes which take place, more complicated.* Even in this view of the subject, there is no want of similarity between the putrid fermentation and the vinous; and a considerable degree of analogy might be traced, very far, were we to consider the particular agents which retard or prevent the two kinds of fermentation; indeed, I am not acquainted with a single agent, or circumstance, the effect of which is limited to the vinous, or to the putrefactive fermentation. They all appear common to both, and also to the fermentation of vegetable matter generally. Such is the effect of corrosive sublimate, of the sulphurous acid, of tannin, of salt, vinegar, alum, spirit

* Adipocire, I believe, like peat, never forms where the atmospheric temperature is high; I have never witnessed the production of either of them in warm climates, excepting, in the instance of peat, on mountainous regions, where the average temperature is moderate, and in that of adipocire, during the cool months, in the Mediteranean.

of wine, sugar, &c. These circumstances, just related, prove some resemblance between the two kinds of fermentation, in their general phenomena,—in the causes which promote and accelerate, and those which retard and prevent them. The leaven which excites vinous fermentation, closely resembles an animal substance in composition and in many of its properties, and is even analogous to fibrin. Like fibrin, it is capable of undergoing the putrid fermentation;* and the fibrin and muscular fibre act as leaven with sugar and water, and give rise to fermentation. Whether the fermentation, thus excited, is similar or not to the ordinary vinous fermentation remains to be shewn. I suspect it is not similar in all its products. Carbonic acid gas is disengaged; the muscle or fibrin is prevented from putrefying,—and if tainted, is corrected; but, I am not aware that alcohol is formed. In one instance, the sugar, by the fermentation, appeared to be converted into gum, powerfully absorbent of water,—but very slightly soluble in water. It was the result of three or four days fermentation of a solution of refined sugar in water, excited by a small portion of beef. It appears too, not improbable, that a principle may

* It also promotes destructive decomposition in vegetable matters in themselves little prone to change. I remember witnessing this effect in a striking manner in a piece of fine table-linen which was sent to the Mediterranean, with other things, and amongst them a bottle of yeast, which burst on the voyage; the damask was to a considerable extent destroyed, but only where the yeast had come in contact with it.

exist, contained in the fibrin of the blood, in muscular fibre, and in the parenchymatous substance of the very putrescible organs in large proportion, which may act as leaven in animal compounds; and I might mention some facts in support of the notion, were I not apprehensive of extending this part of the subject beyond its due limits, and the more so, as I wish briefly to advert to one or two other points of considerable theoretical interest.

After witnessing the change which takes place in muscular fibre, in consequence of putrefaction giving rise to a fluid very like chyme in appearance, I was disposed to ask, may it not be concerned in digestion itself, according to the earliest theoretical notions on this subject? and also, after observing the effects of the putrefactive process in raising the temperature, to inquire, may it not in the stomach and intestines, be one of the sources of animal heat?

Meat slightly putrid, we know, is highly nutritive, and very easily digested. When meat is becoming tender by keeping, it is in an incipient state of putrefaction; the muscular fibre is losing its cohesion; the textures generally in a very minor degree are doing the same, and carbonic acid gas is forming, as I have ascertained by experiment. Now, as we know, that carbonic acid gas is formed in, or disengaged from the stomach and intestines, even when the function of digestion is apparently in its healthy state, it does not seem unreasonable to consider this gas a product of digestion, almost as

much as of fermentation. The other gases, as sulphuretted hydrogen, and carburetted hydrogen, occasionally evolved during the putrid changes, are frequently generated in the alimentary canal; and, the contents of the stomach, and of the whole intestines are seldom, I believe, destitute of ammonia. In relation to this last mentioned product of the putrid fermentation, I may mention, that whenever I have sought for it after death, either in the human stomach, or in the duodenum, jejunum, or ileum, I have always found it; and I need not add, that I have always found it in the large intestines, and in instances in which the *post mortem* examination was made before there were any indications of putrefaction having commenced in the solids or fluids generally, and when ammonia could not be detected either in the blood,* the bile, the brain, or the muscles. The result of the examination of animals just

* In the trials referred to above, the blood and other parts were mixed with quicklime, and the absence of ammonia was inferred, if no ammoniacal odour was perceptible. Recently, I have had recourse to a severer test of the volatile alkali, viz. bringing near the mixture under trial, a glass rod moistened with muriatic acid, so diluted, that no fumes were produced, merely by aqueous vapour. Using this means, in every instance, I have detected ammonia in the blood, even when it has been extracted from the body very soon after death. The degree of effect, however, has very much varied; in some cases, in which the blood-particles appeared under the microscope very little, if at all altered,—the indications of ammonia afforded were only just perceptible: on the contrary, when the colouring matter of the blood was partially dissolved in the serum, and the forms of the blood-particles were altered, then the indications of ammonia were strong.

killed, and which I have made in relation to this point, has not been less conclusive. In one instance, I had a hungry dog, fed at three different times, during the twenty-four hours with fresh beef. Three hours after the last feeding, he was killed. All the meat given him had disappeared, excepting the last meal; and the portions of beef of which it consisted, were very little altered: yet, triturated with hydrate of lime, they gave off a distinct ammoniacal odour,—so did a little whitish chyme in the pyloric extremity of the stomach; the chyme in the duodenum gave it off more strongly; a thin yellowish fluid in the ileum not less so; and the contents of the large intestines in a hardly less degree. I examined the blood, both venous and arterial, the chyle, the bile, and several of the solid parts; but none of them afforded, by the test of lime, the slightest trace of ammonia, not even the kidneys, although the urine abounded in ammoniacal salts, and emitted on the addition of lime a most powerful ammoniacal odour. These circumstances seem in harmony with the old hypothesis—that putrefaction is concerned in digestion.

In opposition, many facts may be stated, tending to shew, that digestion is a specific action, or series of actions,—that its phenomena are vital, or in other words, peculiar, mysterious, and inexplicable according to the present state of our knowledge. Many of the results of the experiments of Spallanzani, and Stephens, are of this character. Contrary to

the idea of putrefaction, the contents of the stomach are generally acrid; and the acidity diminishes almost gradually from the stomach to the rectum. Ammoniacal salts do indeed exist in the stomach and intestines; but in a healthy state, I am not aware, that ammonia, or the carbonate of ammonia has ever been detected in these organs; and consequently, the principal product of putrefaction is found wanting.* Moreover, putridity seems to be corrected in the stomach, and its natural progress stopped. I shall not lay stress on meat introduced in a putrid state into the stomach, having been taken out after a time free from taint, as it may be said, that the putrid particles may have been destroyed, modified or removed, as happens when tainted meat is placed in a saccharine fluid in a state of fermentation. I would rather adduce the fact, which I have witnessed, that muscle which has been in the stomach some time, when taken out and exposed to the atmosphere, does not putrefy more rapidly, but in a remarkable manner resists putrefaction. The substance or matter derived from muscular fibre, undergoing the putrefactive change, is indeed not unlike chyme in appearance, whether viewed with the naked eye, or with the microscope,—both of

* This, it may be said, is owing to the presence of a stronger acid, such as the muriatic, which seems to be, from the experiments of Dr. Prout, an active agent in digestion, and which of course, if free, would unite with any free ammonia formed; so far weakening the above argument.

them abounding in minute particles and containing some globules. But of how little value is such a remark,—and how little to be insisted on! Many different semi-fluids might be mentioned bearing as close a resemblance. It is more to the purpose to compare the matter resulting from putrefaction with chyle. How great is the difference of the two! The chyle is alkaline, and yet contains no sensible quantity of ammonia; there is no reason to suppose that it contains any free carbonic acid; it seems as if it were altogether a new formation. Chyle derived from the muscular fibre of the ox, in the thoracic duct of the dog, or that of the ox derived from grass, consists of a milk-like serum, coagulable by acids, and of lymph spontaneously coagulable, resembling the fibrin of the blood in its manner of coagulating, and gradually contracting, and resembling it also in its manner of undergoing the putrid decomposition on exposure to the air by deliquescing, as it were, or becoming semi-fluid. I will not say how incompatible all this appears to be with putrefaction, but more, how inexplicable, in relation to our imperfect knowledge of the chemical changes taking place amongst the elements of matter destitute of life, or beyond the influence of the vital organs.

If putrefaction is not admitted to be concerned in digestion, the second question asked above is answered as regards it,—“Whether in the stomach and intestines it may not be a source of animal

heat? But substituting digestion for putrefaction, an answer still may be required. The circumstance that some carbonic acid gas is formed in the stomach and intestines, (supposing that which is found, is formed there,) is favourable to the idea of digestion being an auxiliary source of heat. The observations which I have made on the temperature of the stomach in animals, is also in favour of the idea; I have often found it as high as that of the heart, and sometimes, though rarely, a degree higher. The conversion of a solid into a fluid, as of solid food into chyme, or chyle, appears rather an objection to the notion. But, I am not sure that is a valid objection, when properly considered, there being so many remarkable exceptions of the kind, even in instances in which the change of form is greater, as in that of the conversion of a solid into a gas, accompanied by combustion, without condensation, even of the supporter of combustion; and *vice versa*, of the formation of a solid from the union of two gases without change of temperature. Neither, am I aware of any remarkable change of form of an animal or vegetable substance, which is accompanied either by absorption, or by evolution of heat. The fibrin of the blood appears to coagulate without change of temperature; no heat is evolved when serum is coagulated by an acid; none when a strong solution of gum is coagulated by sugar of lead; none when milk is coagulated by rennet.

The influence of the various secreting organs

on the animal temperature is, no doubt, highly deserving of inquiry and of experimental investigation. Were I to draw an inference further from the observations which I have made on the temperature of different parts of the body, I should be disposed to say, that besides the *primæ viæ*, there is no other apparent auxiliary source of heat, (considering the lungs as the main source)—neither the brain, the temperature of which is comparatively low, nor the kidneys, the fluid secreted by which appears in the bladder to be of rather lower temperature than the blood in the heart and great vessels; nor the liver, the temperature of which I have never seen above that degree which might be expected, taking into account its proximity to the lungs, its bulk and the abundant manner in which it is supplied with blood.

In a former part of this work “on the temperature of the human body after death,” two instances are given of an extraordinary high degree of temperature observed in bodies which had been dead a short time; and in both of which the fibrin of the blood had disappeared, as it does in putrefying, and yet the bodies exhibited none of the usual signs of the putrid change having even sensibly commenced. I then concluded in consequence, that the unusual temperature was not owing to putrefaction—or a *post mortem* effect; and, reflecting on all the circumstances of these cases, and on the phenomena witnessed in my experiments on putrefaction, I am confirmed in that inference, and obliged to suppose

that it either arose from the ordinary sources of animal heat being more energetic than usual, or from the cooling process being comparatively inefficient, or else to imagine some peculiar morbid change in the blood itself, on which, in common with the disappearance of the fibrin, the unusual temperature may have depended. In all the instances recorded in the preceding pages, in which, during putrefaction there was a considerable elevation of temperature, it was at a particular period of the process, namely, when it was most active, when the greatest apparent change occurred, before the oxygen of the atmospheric air had disappeared, and when the change was attended with the formation of carbonic acid gas, partly at least, by the union of the oxygen of the air, with the carbon of the putrefying matter. Whether any heat was produced during the after part of the process, when the changes were taking place more slowly, when oxygen was excluded, and carbonic acid gas was formed from its elements in the compound in the same manner as ammonia was formed, may be matter of question. The results themselves were far from conclusive,—rather hostile than favourable to the idea of heat being thus generated. The analogy of vinous fermentation, in which there is a manifest elevation of temperature produced by the arrangements of the elements in new combinations, independent of the presence of oxygen, may be adduced in support of the notion. Were the proportion of

carbonic acid gas generated in the two kinds of fermentation similar, then the analogy would be strong; but, in the putrid fermentation, the quantity of carbonic acid gas formed, independent of the oxygen of atmospheric air, is small, and the quantity of ammonia large. If the formation of ammonia, like that of carbonic acid gas, were usually attended with increase of temperature, then there would be reason to suppose that the fermenting process of animal matter throughout is a heating one. But, I know no proof, that heat is evolved in the union of the elements to form ammonia. In relation to heat, it may be remarked, that azote and ammonia, and the combinations of the latter, and of most of the former, are peculiar and anomalous. Elastic fluids are formed by the sudden decomposition of solid and liquid bodies (the detonating compounds) with little change of temperature, — and *that* an elevation, not a diminution of temperature. Ammoniacal gas unites respectively with the carbonic, muriatic, and fluo-boracic acid gases, forming solid salts, without the production of heat; and as during the slow decomposition of ammonia by electricity, there is no evidence of change of temperature, so equally there is want of evidence of the same in its formation. The probability therefore is, that its elements unite without evolution or generation of heat; and consequently in the putrid fermentation it cannot be considered as a cause of heat.

Appendix,—containing a brief notice of the fatal cases referred to.

No. 1.—Aged 42, died on 25th July, of an abscess in the liver. The body was examined ten hours after death. It was much emaciated; excepting the liver, the lower part of the ileum, and the upper part of the colon, the parts generally were sound. The portions of the intestines alluded to, were slightly ulcerated.

2.—Aged 26; very little emaciated; died on the 14th July, of acute dysentery. The body was examined six hours after death. All the organs were tolerably sound, excepting the large intestines, which exhibited the worst effects of the disease mentioned.

3.—Aged 23; not much emaciated; died on the 2d August, of acute dysentery. The examination was made nineteen hours after death. The pericardium adhered to the heart; there was an abscess in the liver contiguous to the gall-bladder; the inner surface of the gall-bladder was very red, and it contained a serous fluid; the inner coat of the colon was severely ulcerated; the other organs were tolerably sound.

4.—Aged 35; rather emaciated; lower extremities oedematous; died on the 3d July, of disease of the heart and brain. The examination was made ten hours and a half after death. Both sides of the heart were much thickened, and their cavities en-

larged; the central parts of the cerebrum were very much softened; the other organs, with slight exceptions, were tolerably sound.

5.—Aged 26; rather emaciated; died on the 7th August, of acute dysentery. The examination was made six hours after death. No lesion was discovered worthy of notice, excepting in the large intestines, which were very extensively ulcerated, &c.

XVI.

EXPERIMENTS AND OBSERVATIONS ON THE MACERATION IN WATER OF DIFFERENT TEXTURES OF THE HUMAN BODY.

ALTHOUGH the process of maceration by means of water has been long in use amongst anatomists in making preparations of particular parts, especially of the more solid ; and though, of late years, many observations have been collected bearing on this operation, yet I am not aware that any inquirer has yet engaged in the subject systematically, — has attempted to investigate it through the changes which it produces, or even to ascertain with any accuracy what changes really are produced. Nor is this neglect surprising, considering the circumstances attending the maceration of animal matter, — the difficulty of procuring subjects, — the conveniences requisite, — the time required, — the great attention necessary to observe the fleeting phenomena, — the disgusting nature of the experiments, — and the idea of danger of health attached to them.

It was when stationed in Malta, in the year 1828, that I was induced to engage in the investigation for the purpose of removing doubts, and of gaining information which was not to be collected from books. The results then obtained, were published in the 41st volume of the Edinburgh Medical and Surgical Journal; though they were far from being so finished as I could wish, I again bring them forward, with the hope, that as a contribution to anatomy and animal chemistry, they may be of use.

1. — *The Circumstances attending the Experiments.*

The experiments were all made in the laboratory of the General Hospital,—formerly the hospital of the Knights,—a room, by its equable temperature, well adapted for the purpose, and they were carried on during a period very nearly comprehending twelve months. The water used for maceration was aqueduct water, that of the town of Valetta, which appears to be very much the same in quality throughout the year; and seems to be variable chiefly in temperature, from being conveyed a distance of about eight miles, and a considerable part of the way, running very slightly protected from atmospheric influence. During the spring months its temperature varied from about 55° of Fahrenheit to 65°; during the summer from about 65° to 75°; during the autumnal from about 75° to 65°; and

during the winter from about 65° to 55° . In the cool season, it was always a few degrees above the temperature of the room; and in the hot season a few degrees below it. But as the water was changed every day, or every second day at furthest, and as a gallon or two was generally used for maceration, the alteration of the temperature of the water during the progress of the experiments was immaterial and undeserving of particular attention. I have said that the quality of the water was apparently always much the same. This I have not proved by analysis; it is conjectured from considering that the aqueduct is fed by springs in limestone rock which are perennial, and very little affected by the seasons. In July, just before I entered on the inquiry, I particularly examined the water. Its temperature then as it flowed from the pipe into the room, was 73° , its specific gravity was 1008; 100 measures of it contained 4 measures of air, and this air consisted of 10 carbonic acid gas, 62 azote, 28 oxygen.

Its saline and earthy contents were very inconsiderable, not quite $\frac{1}{5000}$ part, and consisted chiefly of carbonate of lime, dissolved by carbonic acid, and of common salt, with a very little lime and magnesia, and fixed alkali in combination with the sulphuric and muriatic acids. The vessels in which the parts were subjected to maceration were glass jars, varying in capacity from one to two gallons. In some instances, they were covered slightly to keep out dust; in others they were uncovered. The subjects

of the experiments were generally suspended in the water by common pack thread, and to prevent their floating when air was generated, they were kept under by a counterpoise of glass attached by the same kind of thread to their inferior surface.

2.—*A brief Account of the Bodies from which the Parts, the Subjects of the Experiments were taken.*

It may be remarked generally, that all the bodies were of the male sex ; and all of them, with the exception of two, of soldiers of our regiments.

1.—Aged 25 ; died unexpectedly on the 24th August, 1828, labouring under symptoms of continued fever of five days' duration. The body was examined about five hours after death. Nothing satisfactory was discovered to account for the fatal event. Blood, in large quantity, had been abstracted, twice, and active purges had been used.

2.—Aged 29 ; died on the 10th October, 1828. The body was examined twenty-two hours after. It was much emaciated ; the cerebrum was in a very soft state, especially its central parts ; the lungs abounded in minute tubercles ; the superior lobe of the right lung contained a tubercular excavation ; and the lower part of the ileum was slightly ulcerated.

3.—Aged 23 ; died on the 16th October, 1828, of "continued fever" (as the disease was called)

of nine days' duration, with some anomalous symptoms. The body was examined fourteen hours after. The cause of death was not ascertained in a satisfactory manner. Some lymph was found effused in the posterior mediastinum, and the lungs contiguous to the mediastinum were turgid with blood, and in a certain degree hepatized; the spleen was unusually large and soft; and the lower part of the ileum was slightly and obscurely diseased.

4.—Aged 24, died on the 4th November, 1829, having had the thigh amputated on the 16th October, and the femoral artery tied just below the *arteria profunda* on the 22nd, on account of secondary hemorrhage. The body was examined twelve hours after death. The crural, and iliac veins of the diseased limb were thickened by deposition of coagulable lymph. The lower half of the thigh bone, which had been amputated, was in a very diseased state, as were also the tendons and muscles attached, and the whole of the knee, almost every part of which was altered; whilst the leg and foot, with the exception of œdema, appeared to be in a sound condition.

5.—Aged 56, died suddenly on the 11th November, 1828. The body was examined fifteen hours after. The heart was unusually large; the semilunar valves of the aorta were partially ossified; the orifices of the coronary arteries were very much contracted; the *arteria innominata*, and the arch of the aorta were enlarged; the aorta was generally diseased;

its inner coat irregularly thickened, and its middle coat corresponding, more or less absorbed.

6.—Aged 18, died on the 12th of November, of dysentery. The body was examined twelve hours after. It was considerably emaciated; and the colon exhibited the appearances usual in this disease in its severe chronic state.

7.—Aged 23, died on the 29th of November, of acute dysentery. The body was examined five hours after. The whole of the left lung, and the superior lobe of the right abounded in small tubercles; and in the former were two or three vomicae. There was a small ulcer in the duodenum. There were several ulcers in the jejunum, and numerous ones in the ileum and colon.

9.—Aged 32, died on the 26th December. The body was examined twenty-two hours after. The liver contained two abscesses. The large intestines were severely ulcerated.

10.—Aged 29, died on the 7th January, of acute dysentery. The body was examined sixteen hours after. There were patches of coagulable lymph on the lower part of the ileum (its villous coat). The colon throughout exhibited the worst effects of acute dysentery.

11.—Aged 20, died on the 7th of August, 1829, of acute dysentery. The body was examined six hours after. The colon was very extensively and severely ulcerated, &c.

12.—Died on the 8th August, of dysentery. The

body was examined nine hours after. The lower part of the ileum was slightly ulcerated. The colon was extensively ulcerated, &c.

3.—*General Results of the Experiments on the Maceration of the different Parts of the Body.*

The experiments on maceration were commenced immediately after the inspection of the bodies. The water, as already remarked, was changed every day, or every second day, and the changes produced, were as frequently observed during the first four or six months, and were noted down from time to time.

Were I to give a minute account of the observations which I have made on the different parts of the body, whilst undergoing maceration in water, a volume would be required to contain them, the details would be extremely tedious, and their utility very doubtful. I shall endeavour to avoid equally the two extremes of great minuteness of description, and of excessive brevity, and confine myself as much as possible to a narration of the more important results. I shall commence with those parts in which the changes produced by maceration are most rapid, and proceed to those in which they are least so, but not confining myself to this order, when a natural connection of the parts, or any other reason, renders a deviation from it useful.

Fibrinous concretion from the heart. The water having been changed daily from the 4th November to

the 12th, the concretion had become quite white, rather soft, and of a slightly putrid odour. On the 22nd it was reduced to a kind of pulverulent sediment. The same change takes place in common fibrin of the blood, whether separated from the colouring matter artificially or naturally, as in the buffy coat, and as in the instance above given of the fibrinous concretions formed in the heart. The time required for the alteration is variable according to the degree of firmness of the fibrin and according to the temperature, being directly as the one, and nearly inversely, as the other ; or in other words, the firmer it is, so much the longer time is required ; and the warmer the weather, or the water, so much the shorter time.

Muscle—heart. (4. 8).—Its muscular substance underwent rapid change ; it very soon became putrid and soft, and was washed away in the form of poul-taceous matter, very like that from the fibrin of the blood ; whilst its enveloping membrane, its valves, tendinous chords, and the great vessels taking their rise from it, were but little altered. With care at a particular period of the process of maceration, it was easy to show that the valves of the aorta and pulmonary artery are connected with the outer fibrinous sheath of these vessels ; and I am disposed to believe that these valves consist of this tissue in greater proportion, than of the inner coat, and that they owe their strength and peculiar properties rather to the former than to the latter. It is worthy of remark,

that the thin muscular substance of the auricles underwent change less rapidly than that of the ventricles. Other muscular parts of the body were subjected to maceration; some of them, as the substance of the tongue, and most of the voluntary muscles exhibited rapid change, like the ventricles of the heart; others, as the muscular coat of the primæ viæ, and especially of the rectum, and of the urinary bladder, more resembled the auricles of the heart in the slow manner in which they underwent alteration.

Brain. (1.)—This organ rapidly becomes soft in water, and forms with it a kind of emulsion, the consistence of which varies of course with the proportion of each. In the state of emulsion it has a peculiar and disagreeable odour, but neither fetid nor ammoniacal. As the water cannot be changed, I do not consider it necessary to give here any particulars of the alterations which take place in the mixture left undisturbed for a considerable time.

Spleen. (6. 7. 8. 10.)—It became soft very rapidly according to circumstances of temperature, &c. in a space of time varying from twenty-four hours to three or four days. When an incision was made into it and gentle pressure applied under water, the softened matter (very like that from fibrin, coloured by the colouring matter of blood) flowed out, fetid, and mixed with some air bubbles; and when it was softened throughout its mass, the operation of pressing it gently being continued under water, the whole of the softened matter was expressed, and nothing

distinctly remained but the vascular structure of the spleen, contained in its enveloping membrane, and presenting a beautiful and instructive appearance, the vessels anastomosing very freely, and in a certain regular manner, and retaining their strength very little impaired. Here I may remark, that with the spleen thus prepared, I have often endeavoured to discover the cribriform structure, of late years attributed by some anatomists to the splenic veins. No where could I observe this structure; every opening in the sides of the veins which I could detect, was an opening into a branch. After the separation of the soft matter, when the maceration of the spleen was continued, it slowly underwent change, and wasted away; the more delicate structure disappearing first, and the trunks of the vessel, and the including membrane enduring longest, much in the same manner as the larger vessels generally, and the fibrous and serous membranes, which will be considered hereafter.

Pancreas. (2. 3. 7.)—This organ softened nearly as rapidly as the spleen. When gently pressed under water, the soft fetid matter was separated; and after two or three days successively renewing the pressure, nothing but its vascular structure and its duct remained, which latter was of great strength, and resisted further maceration for a long time.

Parotid gland. (6. 9.)—It softened much in the same manner as the pancreas, and the poultaceous matter derived from it was very similar. After the

separation of this, there remained a tissue of strong cellular texture, of vessels, nerves, and the branches of the duct, which, the maceration being continued, gradually softened and were very slowly decomposed. The duct itself resisted decomposition very powerfully, and so did a portion of the lining membrane of the mouth, in which it terminated, and from which in minute structure it was not distinguishable.

Thyroid gland. (8.)—It underwent the same changes as the parotid, and in about the same time. When the poultaceous matter generated was expressed, there remained also a tissue of nerves, and cellular texture.

Prostate gland. (12.)—This organ changed rapidly, and was soon reduced to a soft poultaceous mass of a fawn colour, in which a vestige only of cellular tissue could be detected, and the remains of some small vessels.

Kidney. (4.)—It gave off a good deal of the colouring matter of the blood, and was soon reduced to a soft mass. After gentle pressure under water, there remained only the infundibula which had a delicate vascular structure, a few large vessels, its enveloping coat, and the lining membrane of the pelvis, which did not resist prolonged maceration for so long a period as the vessels and membranes of the spleen.

Liver. (3. 4. 8. 9.)—It underwent change pretty rapidly, and in a manner not unlike the kidney. The water was first discoloured by blood, next by grey

poultaceous matter derived from the softening of the substance of the viscus. After maceration for a month or six weeks, there remained little besides its vessels, and the branches of its duct. These resisted decomposition much longer, especially the larger vessels, and the hepatic duct, and some of its branches, to which adhered, for a considerable time, very minute grayish or brownish granules.

Lungs. (3. 4.)—The substance of the lungs softened pretty rapidly, and was easily broken up. A portion of it well washed, displayed very prettily the vascular structure of the organs; after about a month or five weeks, little remained but the larger vessels, (some of which retained their minute ramifications,) and the pleura comparatively little altered. Where there were tubercles, the alteration of softening was quickest, and it commenced with the tubercles. It was very difficult to distinguish, during the progress of change, between the bronchia, and the blood vessels, and decide which underwent change most rapidly.

Arteries—Aorta, (4. 5. 10.) Pulmonary Artery, (10.) Coeliac Artery, (8.) Femoral Artery, (4.)—These vessels underwent change very slowly. The different coats gradually softened, the inner and outer coat first. The inner coat became as it were mucilaginous, and was washed off or removed by the gentlest friction. The outer coat resisted longer; it finally became disintegrated, and reduced partly to a kind of mucilage-like matter, and partly to its

component fibres not distinguishable except on very minute examination of the debris. The middle coat became soft later than the preceding; it wasted irregularly, became full of holes and gradually fell to pieces. During the change, the outer coat assumed a brownish hue; the hue of the inner coat was little altered; the middle coat assumed a bright pink colour. The different arteries exhibited some varieties of change; the pulmonary artery underwent it slowest; the aorta of No. 5 which was diseased, also underwent change very slowly, and in a peculiar manner. The inner coat separated from the middle, and the middle in places from the adjoining fibrous coat, and the middle coat where thickest separated into laminae. These alterations took place in a space of time varying from six weeks to eight months. It may not be amiss to remark, that by the fibrous coat of the artery, I mean that which is next to the middle, and between it and the loose cellular texture, the envelope of the vessel in which there is generally some fatty matter, and which appears to be even less liable to change than the fibrous, but subject to the same kind of alteration.

Veins—Vena cava (4. 5.) — It gradually softened; the outer cellular or fibrous coat preceding a little in its change the proper coat or coats of the vein. The softening and wasting process went on irregularly; one portion suffering greatly, and another adjoining very little; and the kind of change was not always the same in the same texture; here a spot

became soft, semi-transparent and depressed; there elevated and mucilaginous. In some places it was discoloured brown, in others greenish. Gradually it became extremely thin and worn into holes,—and the change continuing, it was reduced to shreds and particles.

Thoracic Duct, (8.)—It slowly underwent change like the vena cava.

Nerves.—I have made but few observations on this subject, and these have been confined to the crural nerve (4.) Its theca gradually softened; in about two months the enveloping and connecting tissue constituting the theca, was partially reduced to a brownish mucilaginous state, which was detached on the slightest friction, and then the nerve separated into its component filaments. These filaments slowly lost their strength, became as it were mucilaginous, and fell to pieces.

Pia Mater, and Arachnoid Membrane, (11.)—The latter membrane in this instance was slightly opaque, and consequently the better fitted for observation. In their change from maceration, the two membranes kept equal pace; they slowly lost their strength, in some places more so than in others; holes formed in them, which gradually increasing in number and size, in about two months reduced them to shreds. The small vessels ramifying through the pia mater, wasted and disappeared much before the larger ones; these latter remained to the last, but perforated, and undergoing the same kind of change as the membranes themselves.

Theca Spinalis, (8.) — It underwent change slowly. In about six weeks it presented a very varied appearance ; in some places eroded as it were, and become very thin ; in others, holes had formed ; close to them there was a conversion into transparent spots ; and generally there was a fibrous vegetable growth adhering to the surface from which it had sprung. The same kind of change gradually proceeding, the membrane was reduced to shreds, and these ultimately to particles of a fawn colour easily suspended in water.

Dura Mater, (2. 3. 4. 10.)—This membrane, during the first two or three weeks, did not appear to undergo any change. Gradually its surface became slimy, either with or without discoloration, but more frequently the latter. In different specimens, the discoloration was very various, as bright-blue, brown, orange, red and greenish. A wasting of the substance of the dura mater attended the formation of the slime, or soft mucilage-like matter ; and, being washed off as it successively formed, the membrane became very thin and corroded into holes. The change proceeded very irregularly. The wasting was chiefly confined to particular spots, as so many centres of action, the adjoining portions remaining little altered for a long time, and having their strength but little impaired. In these portions thus exempted in a manner from rapid alteration, the structure of the tissue was well displayed,—the very delicate connecting fibres yielding

to decay first, the coarser fibres became so much the more apparent. Sometimes the membrane divided into two layers, — occasionally into three or more laminae; but it was always easy in this stage to effect its separation into two. Progressively every part partook of the change, and the membrane was reduced to shreds and particles. Some of these shreds I have examined after having been in maceration eleven months. Internally, when drawn asunder, they displayed the original colour and lustre, and texture of the dura mater, and they themselves appeared to be in progress to ultimate decomposition. It was curious to observe the different specimens placed under similar circumstances undergoing change. Hardly in any two was the change exactly similar, varying either in the rapidity with which it took place, or in the discoloration attending it; and this without any apparent cause, as if owing to difference of composition not appreciable by the eye. During the early progress of the experiments, I have often sought to discover, on the inner surface of the dura mater, the arachnoid membrane; but I was never able to succeed in detecting it. And I may here remark, that all the other attempts which I have made to demonstrate it have been failures; and in consequence I am disposed to believe that it is not reflected over the dura mater according to the ingenious idea of Bichat.

Pericardium, (3. 9.)—It underwent change like

the dura mater; but rather more slowly in the early part of the process, and more rapidly in the latter, and without discoloration. Maceration, I may remark, afforded no evidence of its being a compound texture, consisting of an outer fibrous, and inner serous membrane, of different characters, as Bichat considered it. That it is a fibrous or fibrocellular structure capable of secreting serum, seems certain; but more than this at present appears hypothetical, and deficient in anatomical demonstration.

Cuticle, (4. 10.)—It became detached from the cutis in from ten to thirteen days, and from some parts sooner than from others. It very slowly underwent change. It became discoloured, generally brownish; in one instance partially blue. A slimy matter formed on it attended with softening; and it gradually fell to powder or small shreds. These changes were best observed in the thick cuticle of the sole of the foot, in which instance they occurred in about twelve months.

Cutis, (4. 7. 9. 10.)—The cutis of different parts underwent change somewhat differently. The difference was most remarkable in the very thin cutis, and in the thick; as in the cutis of eyelids, prepuce and scrotum, and in that round the anus, the chin, the abdomen, &c. The very thin cutis resisted change for a long time; the subjacent cellular tissue became distended with water, and then it was extremely difficult to say where this tissue terminated, where

the cutis commenced; in fact the former passed into the latter, and the latter seemed to be merely the boundary surface of the former, the fibres only more closely compacted than in the loose tissue, and in the instance of the scrotum and the cutis, of the penis, impregnated with brown colouring matter. Maceration being continued, gradually the surface became covered with slimy matter, a softening took place, the cutis became thin in spots, and holes formed; and at the same time the subcutaneous tissue lost its strength, became slimy, its fibres separated, and ultimately both the cutis and subjacent tissue were reduced to a sediment of shreds and particles. The thick cutis underwent change more rapidly at first, and slower afterwards. In a short time, as in about a fortnight or three weeks, it became discoloured, of a bright pink or yellow hue, and it exhibited a cribriform structure, very many small holes having formed in it, arranged in a pretty regular manner, penetrating through the cutis. What remained, constituting this cribriform texture was decidedly fibrous. The fibres were more or less coarse and interwoven, and had the colour, lustre, and general appearance of the fibres of tendons, or of the dura mater when separated by maceration. This rapid change was most distinct in the cutis of the verge of the anus. It may be remarked generally, that the more the integuments abounded in the channels of hair and in fatty follicles, so much the more rapid was this first change

or conversion into a cribriform structure. After it had formed, further change was very slow, and in a manner very analogous to that of the thin skin, or of the dura mater and pericardium, and required even longer time for its completion. Between the two extremes of very thick and very thin cutis, there are parts intermediate in degree of thickness, and intermediate also in relation to the follicles, &c. which they contain, as the skin of the inside of fore-arm, and that covering the shin-bone, that of the ear not including its lobe, &c.—and these parts observe also a kind of medium in their rapidity and manner of change. When examined in a certain stage of the process, they appear to consist of the same kind of tissue as the subcutaneous, only a little more condensed; and to the thickest skin, the same remark relative to texture is, I believe, justly applicable.

Alimentary Canal, Mouth, and Fauces, (7.9. 10.)—Different parts of the lining membrane of the mouth and fauces underwent change with very different degrees of rapidity. Where the ducts of numerous mucous glands pass through the membrane, and where the membrane is not extremely thin as in the roof of the mouth, it soon acquired a cribriform texture like that noticed under the head of the cutis, and thus perforated, it underwent further change slowly. Where comparatively few mucous glands are situated in the subjacent tissue, and where the membrane is comparatively thick, as in the side of the mouth, and especially in the neighbourhood of the entrance

of the parotid duct, there from beginning to end it suffered change very slowly. Over the tonsils it experienced the first or cribriform change very rapidly. The glands constituting the tonsils, were soon converted into a semifluid fetid matter, which was removed by washing, and there remained only a tissue of strong white fibres. Where the membrane was very thin, and at the same time penetrated by numerous ducts of mucous glands, as under the tongue, and especially on each side of its frænum, and in the back part of the fauces, there it underwent change rapidly, and it was soon reduced to a mucilage-like and shreddy state. The change which slowly took place in the lining membrane of these parts, bore a very close resemblance to that experienced by the cutis,—a slimy or mucilage-like matter formed,—the part became irregularly corroded and worn into holes,—it lost its strength, and the whole was gradually reduced to shreds, and these ultimately to particles, which indeed throughout the change were constantly forming, and as they formed were removed in changing the water. And I may remark that the tissue subjacent to the lining membrane, experienced the same kind of alteration; and the same kind of observation which induced me to consider the cutis as the external surface of the tissue connected with it, leads me also to infer that this membrane is an analogous boundary of the texture beneath it.

Tongue, (7. 8.)—I have not alluded above to the epithelium of the lining membrane of the mouth;

it appears to be very similar to that covering the upper surface of the tongue. Like that of the tongue, it varies in thickness and distinctness in different parts. It is very thick on the roof of the mouth, and on the anterior and superior part of the tongue; less thick on the sides of the mouth, or middle part of the superior face of the tongue; hardly distinguishable on the surface of the fauces, and the back of the tongue, and not distinguishable on the floor of the mouth under the tongue, and on the inferior surface of this organ. About the third or fourth day, this epithelium began to come away, or was easily detached; but from no part as a continuous membrane, rather like a curd-like substance, especially from the roof of the mouth. From the tongue, particularly the apex, it might be detached in small portions, having some resemblance to cuticle, and bearing the impression of the surface to which it had adhered. After the separation of its epithelium, the next remarkable change in the tongue from maceration, occurred in its muscular substance, which soon became soft and fetid, and in this poul-taceous state it was soon washed away in changing the water, when gentle pressure was at the same time made to it. After the removal of this part, there remained its case, as it were, which varied in texture in different parts. On the under surface it was a delicate membrane, apparently composed of cellular tissue, and which soon wasted away. On the upper it had a mixed character, as if composed of tendon-like fibres, and of cellular tissue,—the for-

mer prevailing most at and towards the apex, and the latter most towards the base, and round the foramen cœcum, and in the anterior part surmounted by papillæ, which, minutely examined, themselves appeared to be bundles of fibres. These papillæ, and this fibrous and cellular tissue slowly underwent a change very like that described when treating of the dura mater, skin, &c.

Œsophagus, (7. 9.)—In the inferior part of the œsophagus, and especially just at its termination, an outer membrane or epithelium, somewhat similar to that of the mouth was (as it generally is) distinct; and it separated in the same manner. Higher up it became very indistinct; and I was not able to trace it in a satisfactory manner in the upper portion, and through the pharynx, where, as I have already observed, its appearance is obscure, and its existence often doubtful. During continued maceration, the muscular coat of the œsophagus first disappeared. The inner or mucous coat gradually became soft and corroded. In about two months in winter, its upper part was reduced to shreds, which, stretched under water, had the silky lustre, and general appearance of cellular tissue, or of very delicate fibrous structure. The lower part was stronger, and resisted disintegration a little longer. The sub-mucous tissue also resisted it a little longer; ultimately it was reduced to the same kind of debris, composed of fibres and altered particles discoloured brown.

Stomach, (4.)—During the first eight or nine days there was no distinct softening or change of any part of this viscus. About the twelfth day, the mucous membrane was softening in some places, and was easily abraded from the subjacent cellular layer. This layer, where the mucous coat was removed from it, had the appearance of peritoneal membrane. Its tissue was considerably distended with water. The muscular, and peritoneal coats were of a light pink hue. About the eighteenth day, the mucous coat was soft and slimy, and when gently rubbed, was readily detached. About the thirty-fifth day, the mucous coat was slimy, and of the weakest consistence; in many places eroded. The adjoining cellular coat, of a fibrinous reticulated appearance, white and of a silky lustre, had lost much of its strength, and was softening. The muscular coat was not every where distinct; where distinct, there cellular tissue appeared intermixed with it, and it was reddish, and extremely feeble. The peritoneal coat, externally light brown, internally white and silky, was also softening. On the forty-fifth day the change was in progress in all the tissues; and on the seventy-second the disintegration was nearly complete; and it was difficult to recognise any of the coats or layers, excepting the submucous, which, though almost deprived of coherence, yet was still pretty distinct; and the larger vessels passing through it were also distinct, but in the same soft weak state.

Small Intestines, (3. 10.)—These parts under-

went changes very similar to those of the stomach, but rather more slowly. On the thirty-sixth day, in one instance, the surface of the mucous coat of the jejunum exfoliated in soft brown laminae, and left exposed a fresh surface in which vessels and fibres might be seen, presenting a very delicate appearance, derived from the submucous tissue, and terminating seemingly in the mucous or inner coat, in the form of loops, especially in the valvulae conniventes, where this configuration was most distinctly displayed, and forcibly gave the idea, that the mucous coat is chiefly formed of vessels, ramifying from and into the submucous cellular tissue.

Large Intestines, (3. 8.)—The changes which they experienced were also of the same kind, but were even slower in taking place. The submucous cellular tissue, especially of the rectum, where it is very thick, retained its form, and considerable strength, after two months' maceration. The mucous membrane within the verge of the anus, like the skin at the line of junction, changed more rapidly than the contiguous part of either texture; and in about twenty-one days, in the cool season, when the cuticle was in the act of separation, the softening process had even then commenced. Minute cavities formed both in the mucous coat and in the cutis, which increasing in number and extent, in about a month nearly effected a separation of the two membranes. This greater tendency to decompose in this place of junction, I am disposed to attri-

bute to the mucous membrane, near its termination, having beneath it an unusual number of glandules or follicles ; and to the cutaneous membrane in the same situation having a large supply in its substance of reddish granules, which perhaps are follicles or glandules which secrete a fluid peculiar to the part. It may be remarked that I could not follow the cuticle beyond the inner verge of the anus,—that, when it terminated, it seemed, as it were, to overlap, and to have no junction with any epithelium, similar to that which in the mouth connects the inner surface of that cavity with the outer surface of the lips.

Gall Bladder and Biliary Ducts, (2. 4.)—Both the gall bladder and the biliary ducts were slow in suffering change from maceration, especially the latter. After twelve days, in the instance of No. 4, they had undergone no apparent alteration. After eighteen days, when a portion of the liver and pancreas attached to them were in a state of disintegration, the ducts did not appear to be at all changed ; but the inner coat of the gall-bladder was softening, and, when gently rubbed under water, yellow particles were detached from it. After twenty-three days, the inner coat of the gall-bladder was slimy, and coming off in flocculi, whilst the ducts were not materially altered ; their peculiar structure remained distinct, the lancet-shaped valvular processes at and near the mouth of the common duct, and the transverse, and oblique fibres and chords, which some anatomists have called spiral laminae, in the cystic duct,

were as distinct as at first, and not perceptibly weakened. The appearance of the inner surface of these channels bore a strong resemblance to the sinuses of the dura mater; and their general properties, and the manner in which they long resisted decomposition, and the manner in which they very gradually yielded to it, confirmed the analogy. After thirty-five days, neither the hepatic, nor the cystic, nor the common duct, nor a portion of the pancreatic duct attached, was altered, except very slightly; the lancet-shaped processes above alluded to were gradually wearing away. The cellular, or cellular-fibrous tissue enveloping the gall bladder and duct had become very soft; yet when stretched under water, its fibres had the peculiar tendinous lustre and appearance. The inner reticulated surface of the gall-bladder still retained its orange hue; in many places it was of mucous consistence, or reduced to a slime-like state. After fifty-four days, the ducts exactly resembled the dura mater; the lancet-shaped processes were still distinguishable; the gall bladder had lost almost entirely its inner reticulated surface, — that commonly called its mucous coat; and there remained little but a tissue of fibres, which when stretched under water had a silky lustre like tendinous fibres, and retained a greenish yellow tinge of bile. After about three months there was little further alteration, and even after four no very considerable one; the ducts gradually wasted like the dura mater; a mucus-like

substance formed on them ; they became eroded as it were, and more in some places than in others, and worn into holes ; and the investing tissue of ducts, and gall-bladder became soft and incapable of resistance ; and thus gradually in from about six to eight months, they experienced a total disintegration, and were reduced to shreds and particles of a greenish hue.

Air Passages, (7. 9.)—Different parts of the aspera arteria suffered change very variously in relation to time. In the instance of No. 7. on the twenty-first day, there was an appearance of erosion of the epiglottis, and a wasting of the lining membrane of the trachea, portions of which were in a shreddy state ; whilst the whole of the lining membrane of the larynx appeared to be unaltered, excepting the sacculi laryngis, the loose tissue in which had become distended with water, producing an appearance like oedema ; and the same was observable round the rima glottidis. In the instance of No. 9. on the twenty-eighth day, the erosion of the epiglottis was very similar, but the alteration in the trachea less. On the thirty-second day, (in this latter example, to which I shall now confine myself,) the lining membrane of the epiglottis was perforated with small holes, corresponding to glandules underneath, undergoing decomposition, situated in the substance of the cartilage, and in the connecting tissue, performing the part of its perichondrium ; and similar minute perforations were formed in the lining mem-

brane of the trachea, most remarkably in the back or membranous part of the canal, and I believe from the same cause, namely softening and disintegration of the mucous glandules, situated in the tissue next to the lining membrane, and communicating by ducts or pores with this membrane. At this time the muscles of the windpipe, and the thyroid gland were in a poultaceous state; but the inner membrane of the larynx still remained apparently unaltered. On the fifty-ninth day, the membrane of the epiglottis had almost entirely disappeared; its cartilage was full of perforations, and its margin was much worn and had become soft. The lining membrane of the larynx was here and there slightly eroded, as if ulcerated. When a portion of it was dissected off, and stretched under water, it had the appearance of a fibrous membrane. The chordæ vocales retained their original appearance; and their strength was not apparently decreased. The inner membrane of the trachea had nearly disappeared; the shreds of it which remained had also the character of fibrous membrane when stretched under water, although extremely delicate. The back portion of the lining membrane had most distinctly this character, and it had now acquired a cribriform texture, from the many cavities formed in it. Of the changes which successively took place after the fifty-ninth day, I have not any notes. I may remark generally from recollection of observations made on other specimens, that the chordæ vocales, the lining

membrane of the larynx, the tendon by which the epiglottis is attached to the chordæ vocales at their junction, were the last parts that underwent decomposition; and that they underwent the change in a manner analogous to the dura mater, and the tendons of muscles. The cartilage which yielded first was the epiglottis; next the arytenoid, and the cartilaginous rings of the trachea; and, lastly, the annular and scutiform cartilages; and they underwent the change principally by a process of softening, becoming slowly converted into a mucus-like substance. I may mention that I endeavoured to trace an epithelium from the tongue into the larynx, but in vain; also that I sought for a villous coat in the air-passages, but in vain. The lining membrane appeared to me to be of the nature of a fibrous tissue throughout, and strictly not a distinct membrane,—merely the boundary surface of the lining tissue and the component parts. I could detach no membrane from the chordæ vocales; their surface appeared to be the same as their substance,—closely and very firmly connected tendinous fibres. Nor could I detach a portion in the form or semblance of a membrane from any part, except corresponding to a comparatively loose fibrous tissue, in which or beneath which, are situated the glands, which give the surface the character of a mucous membrane.

The Passages of the Nose and Ear, and the Cavities connected with them, (10.)—I have not paid

minute attention to these parts during a long period of maceration. The few observations which I have made in one instance, I shall communicate. Where the lining membrane of the nares joins the cutis, and where the cuticle of skin terminates, there, on maceration during fifteen days, I have noticed a kind of epithelium, at that time of curd-like consistence, not dissimilar to the covering of the roof of the mouth, admitting of being scraped off, rather than detached as a continuous surface. As it has proceeded inwards, it has rapidly diminished, and I have not been able to follow it more than half an inch in a satisfactory manner. The lining membrane of the nasal passages and cavities, has resisted decomposition for a considerable time, and in some measure according to its degree of thickness. The same remark is applicable to the lining membrane of the passages of the ear. The membrane of the eustachian tube, being comparatively thick, has borne maceration a very long time; whilst the extremely delicate membrane of the tympanum has disappeared rapidly. Both the lining membrane of the nasal passages, and of the ear, have exhibited generally, during maceration, the character of a fibrous tissue, with mucous glandules enveloped in them, or subjacent to them. In the former I have been much at a loss to distinguish between the periosteum of the cavities, and the lining membrane of the cavities; and in the frontal and maxillary sinuses, and those of the sphenoidal bone, I am

much disposed to believe that they are one and the same membrane, with mucous follicles intermixed between their fibres. The membrane lining the tympanum, of such extreme delicacy and vascular, bears a greater resemblance to the pia mater than to either a mucous membrane or ordinary periosteum,—yet it seems to perform the function of both, though I am not certain that it is provided with any mucous glandules, or moistened with mucus. The membrana tympani appears to be continuous or connected with it; and also continuous, or connected with the cuticular lining of the external passage, which is covered with cuticle, and which seems to serve for periosteum to the bony ring of the meatus auditorius.

The Eye.—The observations which I have made on this organ and its appendices during maceration, have also been confined to one instance, (No. 10.) and have been more hasty than I could have wished. During maceration, when the cuticle has separated from the eyelids, at the inner boundary of the cilia, I have not been able to detect any, the slightest epithelium, on the conjunctiva. The conjunctiva has undergone change like cellular tissue; the sclerotic coat like dura mater, but more slowly; the cornea has become of a milky appearance, has gradually softened, and has wasted away more rapidly than the sclerotica. The lens has become soft, and like thick mucus; and the change has gradually proceeded from the circumference to the centre;

and the central part previous to softening, has assumed an opalescent and greenish hue. The membrane of the vitreous humour, before softening, has, like the cornea, acquired a slight degree of opacity, and in consequence has appeared so much the more distinct; it has resisted decomposition many weeks. The lachrymal gland has undergone decomposition very like the parotid; and the lachrymal ducts and sac very like the lining of the nares.

The Urinary and Seminal Organs. The bladder, (4. 7. 6.)—In the instance No. 4. this organ was under observation seventy-two days. Marks of change began to appear on the sixth day, chiefly in the muscular coat. On the twelfth day, this coat was of a light pink hue, whilst the inner coat was white, firm, and apparently unaltered. On the eighteenth day, the inner partially began to soften. On the twenty-third day, all the different coats were softening; the peritoneal was white, the muscular pink, and the inner coat yellowish, with slimy matter of the same hue adhering to it. On the thirty-fifth day, they had all become very soft, and capable of resisting little force; and both the peritoneal surface, and the inner surface were slimy. On the forty-fifth day, the muscular coat was reduced to pale soft shreds; portions only of the inner coat were distinct; and these when examined under water, had the lustre and appearance of the debris of a serous or fine fibrous membrane. The adjoining cellular layer, and the cellular layer con-

necting the peritoneal coat and the muscular, and the peritoneal coat itself, were much in the same state. On the seventy-second day, very little remained; the small residue not lost in changing the water was in the form of a debris, resembling that of a cellular, serous, or fibrous membrane; and I allude to the three, not knowing well how to distinguish the debris of the one from the other. The ureters, it may be remarked, resisted disintegration considerably longer. During the whole period of change, they very much resembled the biliary ducts, and gradually wasted like fibrous tissue. Whilst the maceration was in progress, I sought in vain for a membrane deserving the name of epithelium in the bladder and ureters; and I was as unsuccessful in my search for villi on their lining membrane. I sought also for mucous glands or follicles, but could not detect them in a satisfactory manner in the bladder, excepting a few close to its orifice, in the small triangular space. In some instances they appeared indistinctly in the ureters, in others not. I must confess, I have doubts if the lining membrane of the bladder deserves the name of a mucous membrane. In the dead body, I have never found mucus adhering to it; and it is easy to conceive that the mucus which is contained in the urine may be derived from the glandules, or follicles above alluded to, and from those of the urethra. I express these doubts with hesitation; and chiefly because it appears wrong to take for granted that which is not

proved; and no less so to place much confidence in analogical reasoning, in opposition to or not supported by, the simple evidence of the senses.

Testes, and their Membranes, (7. 12.)—In the instance of No. 7, after eighteen days' maceration, one testis suspended in water deprived of its tunica albuginea, had become very soft and weak; its structure was well displayed, and its capillary tubes might be drawn out to some length. It appeared to be yielding pretty rapidly to decomposition,—but to my surprise it continued pretty nearly in the same state more than a month. On the sixtieth day little of it remained, and that was extremely soft. The tunica albuginea detached from one testis, underwent change rather more slowly than the other not detached. Both became corroded, gradually perforated, covered with a mucus-like substance, and a fibrous vegetable growth. They underwent change more rapidly than the tunica vaginalis. On the eighty-ninth day, they were full of holes, and what remained was too weak to support its own weight; it was almost of a mucilaginous consistence, and had acquired a certain degree of transparency. At the same time the tunica vaginalis, and the tissue of scrotum, though somewhat wasted, still retained a certain degree of strength, and when stretched under water, had very much their original appearance,—the appearance of a tissue of very delicate fibres with some coarser ones intermixed, of a silky, or tendinous lustre. During maceration, I attempted

to divide the tunica albuginea into two or more layers, but could not succeed. The vas deferens underwent change very like the tunica albuginea; its inner surface, near its termination, which is a reticulated structure, composed of fine tendon-like fibres, resisted disintegration longest. The vesiculæ seminales too underwent a very similar change, and their inner fibrous reticulated tissue also resisted change the longest.

The Penis, (6. 7.)—On immersion, this organ became distended with water, especially the loose prepuce, and the cells of the corpora cavernosa, and spongiosa. At the temperature of about 60°; after maceration of eight days, the cuticle almost ceased to adhere, and might be detached most easily. I carefully traced it to the mouth of the urethra; about a line inside of the orifice it terminated, much in the same manner as the cuticle terminates in the anus, overlapping the membrane of the urethra. I have endeavoured to find an epithelium as a distinct membrane in the urethra continuous with the cuticle, but in vain; nor have I succeeded in finding any villi in this canal. Few parts of the body resist maceration longer than the penis; and all its different textures are in a very remarkable manner little liable to change, as its skin, its nerves, and vessels, the lining membrane of the urethra, and more especially its ligamentous sheath. Many months are required for the disintegration of any one of these parts; and some of them after twelve

months' maceration in water, changed daily, or every second day, still retained their form, and were only partially corroded or wasted; as the skin of the glans penis, and its internal structure, and as the ligamentous sheath of the corpus cavernosum. The process of change in all these parts appears to be very similar in kind to that of the dura mater, though much slower, excepting the upper part of the urethra, where that membrane is very thin, and where contiguous to it are numerous mucous glandules, which undergo change rapidly, and seem to accelerate its change. The process of change appears to be by the formation of a mucus-like substance, which in the penis I have noticed of very various colours, as pink, dull-brown, bright-brown, orange, and green. It is preceded by softening of the parts; it takes place chiefly at the surface, or in the portions most exposed to the action of the water; it is not equable; but, as in the dura mater, spreads as it were from certain points or centres, reminding one of ulceration,—the part affected seemingly protecting the contiguous part within certain limits from change.

Tendon, (4, &c.)—This part under my observation has undergone change very slowly; and the less mixed it has been with other tissues, so much the less prone has it been to alteration. A portion of the tendo achillis of a limb amputated on the 16th October, 1828, after constant maceration for many months, was little changed; the longitudinal fibres were still strong; and when the adhering

brown mucus-like substance, which had formed in small quantity on the surface, was rubbed off, they exhibited their original colour and lustre. The connecting tissue of the fibres had been either destroyed or rendered very soft, and in consequence the longitudinal fibres were easily separated. Tendons, at their origins and insertions, where most mixed with other textures, change more rapidly; but even in these it seems to be rather a disintegration or separation of fibres, than a decomposition of them, and amongst the debris tendinous fibres may always be found, on careful examination, little altered; at least so it has happened in my experiments. The change to which tendon is liable, appears to be the same as that noticed under the head of the penis.

Synovial Membrane, (4. 10.)—This membrane has lost its firmness like serous membrane, and its surface has become slimy. With the exception of a few tendon-like fibres, or shreds composed of those fibres and cellular tissue, it has disappeared during a course of maceration of about four months; and the fatty appendices have been apparently converted into adipocire.

Perichondrium and Periosteum.—Both these tissues have undergone change in the same manner, and at the same rate in relation to time, as the dura mater, where they have been detached by dissection from the parts which they enveloped. When they have not been detached, their change has been modified by the influence of the contiguous surface,—

sometimes apparently retarded, and sometimes apparently accelerated. These remarks apply chiefly to the perichondrium of the cartilages of the ribs, and the periosteum of the cylindrical bones. The perichondrium and periosteum of other parts, according to a variety of circumstances, suffer alteration variously, and especially according to their degree of thickness; thus, when very thin and loose resembling cellular membrane, the periosteum of the lower jaw disintegrates rather rapidly, like the adjoining cellular membrane, from which it is extremely difficult to distinguish it, and to say where the one begins, and the other terminates.

Cartilage, (4. 10.)—From the observations which I have made, cartilage, and fibro-cartilage appear on maceration to undergo slowly a very similar change, and to waste away gradually from being converted into a mucus-like substance. During the change, the colours which I have witnessed in some parts have been various and extremely bright; especially in the cartilage of the symphysis pubis; I have seen it vividly coloured partially brown, orange, blue, green, and yellow.

Bone, (4. 10.)—This structure undergoes change from maceration very slowly, especially the compact bones which contain few vessels and little blood. Changing the water frequently, the animal matter appears to be slowly removed; for there is a constant exudation of a mucus-like substance on the surface, and the brittleness of the bone gradually

increases. Various bones, after having been under observation twelve months, had not lost the whole of their animal matter, not even the most delicate, as the fine plates of the ethmoid, and the spongy structure of the turbinated bone.

Intervertebral substance.—I know of no part of the body so little changed by maceration as this. Portions of it from different bodies, which had been in water from eight to twelve months, retained their form, were very little altered in appearance, and had lost little of their substance. The central soft part, almost of gelatinous consistence in the recent subject, was the part chiefly affected; it, in the time mentioned, had principally wasted and disappeared. The outer layers, on the contrary, suffered least; after twelve months they retained, as well as I could judge, their original firmness; and when the minute quantity of mucus-like substance which had formed on their surface was washed off, they exhibited their original colour.

4.—*General Remarks and Conclusions.*

The changes produced by maceration, or during the process, are referrible, I believe, to three distinct causes. 1st, To chemical causes, giving rise to new combinations of the component elements, and to new productions with the aid of the oxygen dissolved in the water. 2dly, To insects,—those which constitute the family of Infusoria of Linnæus, (many

of which appear to be larvæ) feeding on the animal matter. In many experiments, the larvæ of the common gnat of Malta, has been very abundant and active as a destroying agent, performing in relation to change of form of matter, a part in water similar to that performed by the larvæ of flies on dead animal substances in the air. 3dly, To the operation of vegetation of those obscure plants, belonging principally to the *confervæ* of the Swedish naturalist, and which in my experiments have invariably appeared, if the process has been at all protracted; and commonly either as fibres shooting from the part in straight lines, sometimes an inch in length, or more obscurely, as a fine velvet-like protuberance, or growth resembling a byssus. The former kind has appeared most remarkably on the neurilema, on the cutis of glans penis, on its ligamentous sheath, on the tunica albuginea, and on the bones, especially the spongy bones. The latter kind has occurred chiefly on the debris of ligamentous and fibrous parts.

The first, or the chemical causes, must be considered as the main ones; and the second and third only as auxiliary, and in relation to effect, infinitely inferior in activity. The changes chemically produced, I have not yet attempted to investigate with any minuteness. I suspect they are in most respects similar to those which occur during putrefaction in the open air, modified in a certain manner by the solvent power of the water, and moreover

modified by the removal mechanically by the water of the particles detached, and not dissolved. I have ascertained that the same gases are disengaged from the different textures during putrefaction in the atmosphere, and during maceration or putrefaction in water; and as far as I am able to judge from a slight examination, the insoluble debris, generally resulting in both, does not materially differ. The most striking difference which occurs in considering the two processes is in relation to time, most parts undergoing change more rapidly in the air than in the water, that is, when there is as free an access of air as of water. The difference is probably owing to the water removing a substance which may be called animal leaven, very apt to decompose itself, and to promote the decomposition of animal and vegetable matter with which it may be mixed, and which abounds in, and forms a considerable part of all the textures liable to change, and exists more sparingly in those parts which are slow in changing.

XVII.

GENERAL REMARKS ON BICHAT'S THEORY OF MEMBRANES IN CONNEXION WITH THE PRECEDING OBSERVATIONS.

BICHAT, in investigating the nature of membranes, paid attention to the effects of maceration, and in his *Anatomie Générale*, he has given the results of his experience. In some instances, I perceive the results which I obtained accord with his,—in others they vary slightly, and in some they materially disagree. I shall notice only one or two instances of the latter.

He describes tendon as undergoing change pretty rapidly from maceration,—at least comparatively. He says, it undergoes change more rapidly than cellular tissue. And, he extends the remark to analogous textures, as to aponeurosis, and also to the cutis vera, and to cartilage: he expresses it as his opinion, that if the substance of these textures were disposed in five laminae, like cellular tissue, and not

compacted together, he is persuaded, that a maceration of three or four days would suffice to reduce them to a putrid mass.*

Mucous membranes he describes as peculiarly apt to yield to maceration,—more so, indeed, he asserts, than any other animal substance, with the exception of brain; and he brings forward the remark, without distinction, without exception, as applicable to the membranes generally.†

It would be of little use to attempt to explain the discrepancy of these results,—I mean of those which I have described as witnessed by myself, and those referred to of Bichat. What I have observed, in multiplied instances, convinces me, that pure tendinous fibre, as regards tendency to change from maceration, is remarkably little prone to it; and the circumstances which I have mentioned in relating the phenomena, appear to me demonstrative of it—especially one, viz. when a mass of tendon is distintegrated by maceration, its minute fibres, after a very long lapse of time, may be picked out very little altered.

As regards his remark on mucous membranes, relative to the effects of maceration, it appears to me to be deficient in correctness, from its extreme generalness. The observations which I have detailed, shew how great is the difference in different mucous membranes, and even in different parts of the same canal in relation to the effect of this process.

* Anatomie Générale, i. 162.

† Idem. iv. 21.

When it is considered, how rapidly Bichat pushed his researches,—the extent and the variety of his labours,—the short period of his professional life—whilst one admires his genius, and admires with astonishment his efforts,—one cannot be surprised at his inaccuracies.*

Bichat's propensity to generalization was extreme; it may be considered almost as a passion in him; all his writings are characteristic of it. It enabled him to form brilliant hypotheses,—most extensive general views,—so much the more dangerous on account of their brilliancy,—and the ingenuity,

* This extraordinary man,—according to M. Husson, was born on the 14th November, 1771; according to M. Pinel, on the 22d November, 1772. In his 22d or 23d year, viz. in 1793, he went to Paris to finish his medical education. A month only after his arrival, he attracted the favourable notice of Desault, and became his assistant. This great surgeon died in 1795. Bichat then commenced his public life; he collected and edited Desault's works in an incredibly short time, and that same year gave his first course of anatomy. In 1800, after eighteen months devoted to research, he published his treatise on membranes; the same year his treatise on Life and Death; the following year his *System of General Anatomy*; and the commencement of his *System of Descriptive Anatomy*. On the 22d July, 1802, he died after an illness of a few days, supposed to have been owing in part to inhaling offensive effluvia in carrying on some researches on the putrefaction of animal matter. Thus, in the short period of about nine years he ran his astonishing career as an original inquirer. Pinel, from whose notice of the life of Bichat, these particulars are chiefly taken; states, that during one winter, he opened more than six hundred bodies,—and immediately after gave the results of his observations in a course of lectures on Morbid Anatomy, in which he undertook to shew, that each texture has a peculiar mode of disease forming a part of its peculiar vital character.

and indeed genius displayed in supporting them. No part of his General Anatomy was more elaborated than that embracing the mucous system: it well displays the disposition alluded to. Villi and papillæ (“*villosités* ou *papilles*” he did not distinguish between them) he considered a part of the character of these membranes,—without exception and essential,—evidently guided by analogy, by what is witnessed in certain organs, as in the small intestines, as regards villi, and the tongue and glans penis as regards papillæ: whilst, in point of fact, instead of being universal in mucous surfaces, as he generalized, it is more than questionable, if they are found any where else, at least in man.

In relation to the general doctrines of Bichat on the subject of membranes, reasoning on the results of the experiments which I have detailed on maceration,—the conclusion which appears to me unavoidable, is unfavourable; and, it is supported by the results of other experiments,—by those on putrefaction,—on the action of heat, and of coction, and of such chemical agents, the effects of which I have tried, especially of tannin and corrosive sublimate; and also by various physiological and pathological facts and considerations, — altogether constituting the kind of evidence which Bichat himself, after he had formed his hypothesis, brought forward to support it. Let us briefly consider the principles of these doctrines:—

1st, Relative to pathology; whence the first idea

of a classification of membranes into distinct, definite species was derived; is it true, (adopting his distinction of membranes) that under the influence of disease, the same membrane throughout is uniformly affected; that similar membranes are similarly affected, and different membranes dissimilarly?

2d, Relative to structure;—is it true, that there are certain simple elementary membranes,—as the mucous, the serous, the fibrous, &c. essentially different; analogous in the organic system to the chemical elements, as oxygen, hydrogen, nitrogen, &c. in the inorganic?

3d, Relative to particular membranes;—to take an example; is it true, of the mucous, as Bichat maintains, that they possess an uninterrupted continuity of similar structure throughout?

1st, Of the pathological argument, — originally proposed by Dr. Carmichael Smith, adopted by Pinel, and elaborated by Bichat:—in the extensive generalization to which Bichat carried it, and which was necessary for his grand views, are there not many well established facts opposed to it? If his generalization were correct, would not the spreading of organic disease and its isolation be very different from what it is found to be? Would the inflammation and ulceration of dysentery be confined to the large intestines? Would there be local inflammations, and ulcerations,—not confined to any particular texture in which they commenced,—not spreading

along that texture or membrane,—but extending in a perfectly opposite manner ; in the alimentary canal, for instance, perforating through all the coats into the cavity of the abdomen, giving rise to fatal peritonitis;—in the subcutaneous cellular tissue, to take another instance, not spreading as in diffuse cellular inflammation, through the tissue in which the diseased action commenced, but extending directly outwards, and penetrating through the integuments, as happens in common phlegmon? Would in one disease, various and dissimilar organs be affected,—as happens in so striking a manner in tubercular phthisis,—to the extent, to baffle calculation as to the limit of complication ; and as happens, in the majority of instances of the exanthemata, when they prove fatal, especially in small pox, measles, scarlatina, and erysipelas, the fatal termination of which is commonly owing to the supervention of disease in some deeply seated organ, frequently never suspected during life,—not having been indicated by any well marked symptoms?

The evidence derived from pathology dispassionately considered, appears to me on the whole far stronger against, than for the peculiar views of Bichat. Guided by these views, one cannot proceed a step in the careful examination of disease in connexion with structure without encountering difficulties. I shall notice in the briefest manner only a very few,—and for the sake of briefness in the form of questions. Why is the aorta so frequently dis-

eased and the pulmonary artery so rarely, their structure being similar? Why is the upper part of the duodenum so rarely the seat of ulceration, and the lower part of the ileum so frequently, and still more frequently the vermiform process of the cœcum, all three being glandular, but the duodenum in the most marked degree? Why in tubercular phthisis are ulcers so frequently found in the larynx, and so unfrequently in the trachea? Why is the urethra so little liable to ulcerate? Why are the ureters almost exempt from ulceration?

2dly, On the structure of membranes, viewed as elements,—simple and uniform,—well marked by their appearances and properties, — perfectly distinct from each other, and incapable of being confounded;—in colour, thickness, hardness, density, resistance, &c. nowise similar;—subjected to desiccation, putrefaction, maceration, coction, &c. offering different results,—in brief, according to Bichat's idea, each as well characterised as is a simple substance, or chemical element: on this subject it appears to me there is much want of satisfactory proof, and the weight of evidence is against the conclusion. How little analogous for instance is the lining membrane of the ureters and of the small intestines; of the gall bladder, and of the urinary bladder; of the frontal sinuses, and the other adjoining sinuses, and of the stomach and duodenum; of the stomach and duodenum, and of the larynx and trachea. Some of these in structure, appearance, firmness, the

manner in which they undergo change and are affected by reagents have a greater resemblance to fibrous membranes than to mucous,—especially the lining membrane of the ureters, of the larynx, of the sinuses. In the frontal sinuses, I have not been able to detach a mucous membrane from the periosteum; the periosteum and mucous membrane appear to be one,—the mucus I believe being derived from follicles contained in the tissue of the periosteum. In the ureters, it appears to me very doubtful, that the lining membrane is a mucous membrane. In character it appears to be a delicate fibrous tissue; I have never detected mucous follicles either in its substance, or beneath it, or mucus in its canal; and I have often carefully sought for both.* It would be easy to multiply instances, of the deficiency of well marked character,—subversive, I apprehend, of the generalization in question. In the scrotum, the cutis vena seems to be merely the boundary of the cellular tissue of the scrotum, externally; the one insensibly passes into the other. A similar remark applies to the periosteum of the maxillæ, especially of the inner surface; the membrane there covering the bone, seems cellular membrane only a little condensed, and differs very little in character, in the instances in which I have examined it, from the contiguous cellular membrane.

* In accordance with this it is liable to adhesive inflammation:—in two instances I have known the ureter closed by the adhesion of its lining membrane.

3dly, Take the instance of a particular membrane, and examine it according to the views of Bichat,—as to the continuity and identity of structure,—and see what will be the result in point of evidence; and let it be a mucous membrane as before proposed. If the alimentary canal be selected, how very different will its different parts be found; the œsophagus with its epithelium, without papillæ, without villi; the duodenum with a villous surface, over a glandular structure; the jejunum with its villous valvulæ conniventes, the colon without villi or papillæ, without valvulæ conniventes, possessed of solitary mucous follicles. If the air passages be chosen, the results of the examination of the different parts will hardly be more dissimilar; as of the under surface of the epiglottis compared with the cordæ vocales; or the surface of these compared with that of the trachea beneath; or that of the trachea compared with the surface of the bronchial branches. In the different parts of the mouth, the differences of structure are even more strongly marked, as the upper and under surface of tongue,—the one covered with a thick epithelium, — the other I believe altogether destitute of epithelium. The surface of the amygdalæ presenting fibrous cryptæ, without epithelium; the surface of the palate, a dense fibrous structure, not I believe distinguishable from the periosteum, abounding in glandules covered with a thick epithelium; that of the uvula, delicate and yielding, little more than loose cellular tissue, con-

nected with muscles; that of the cheeks, firm and dense, and like the parotid duct very analogous to a fine fibrous structure. An examination of the eye, would call forth similar remarks: the transparent cornea is continuous with the opaque sclerotica; the conjunctival lining with the lachrymal ducts; how strongly are they contrasted! If from man the examination were extended to other animals, the differences presenting in continuous surfaces would be found to be even more striking,—as in the several stomachs of the ruminating animals,—the gizzard of the gallinaceous tribe,—the œsophagus of many of the tribe of cartilaginous fishes. How totally inimical to the doctrine of Bichat, is the second instance alluded to, in which the continuity of mucous membrane is entirely interrupted, and its place supplied by a horny lining.

M. Magendie, in his excellent notes to Bichat's Treatise on Membranes, has made remarks to the same effect. He remarks admirably on this very subject, "that generalization carried too far creates difficulties. That it is clearly useful to study mucous surfaces as one and the same membrane extended over different organs; but, that when the examination of the structure of the membrane is minutely investigated in detail, marked differences occur. In truth, (he adds) mucous membranes differ essentially, according to the organs, and even the parts of organs to which they belong. The mucous lining of the œsophagus, does in nothing resemble it may

be said that of the stomach or nose. The lining membrane of the small intestines, is totally different from that of the large, or of the bladder of urine; and it is precisely the points constituting the difference which ought to be remarked, much more deservedly so than the points of resemblance, which are very slight. In animals (he proceeds) the characters are even more strongly opposed; in the stomach of the horse, the splenic portion is entirely different from the membranous pyloric portion. Each of the four stomachs of the ruminants offers a very different disposition of structure;—to become acquainted with the function of each, it is necessary to study the peculiar structure of each. In general, it is of much more importance to be acquainted with the differences than with the analogies of things.”*

In this note, M. Magendie, it appears to me, has designated well the defective part of the doctrines of Bichat—generalization,—which was his strength, indulged in to excess, constituting his weakness. His doctrine of membranes may be considered as an hypothesis; such it was originally, and such I apprehend it is still. A few pathological observations originated the notion of distinct elementary membranes; analogy led to the idea that they would be found distinct in structure and properties; experiments and observations were made in expectation of confirming the analogy, and it ac-

* *Traité de Membranes*, Nouvelle edit. revue par M. Magendie.—Paris, 1827.—Note p. 33.

cordingly was in a certain manner confirmed; as M. Magendie remarks, the points of resemblance, which were slight, having been seized,—those of difference, often great and always more important, neglected.

Bichat's theory of membranes often reminds me of the antiphlogistic theory of Lavoisier; as simple, as logical, admitting the premises; and, viewed as an effort of mind, as great an effort of genius; and like it a too hasty generalization, that is, instead of being an induction of facts, it is in many respects a presumption,—the anticipatio mentis, built up in great part *a priori*; and having perhaps the same tendency to shackle the mind and check free enquiry. Bacon profoundly remarks, “Credunt homines rationem suam verbis imperare; sed fit etiam ut verba vim suam super intellectum retorqueant et reflectant quod philosophiam et scientias reddidit sophisticas et inactivas.”

The investigation of the subject of membranes and tissues is undoubtedly one of great importance, and deserving of most careful attention. Something has been done in the inquiry, but I apprehend, little in comparison with what remains to be done, to bring it to a satisfactory conclusion. In proof, it may be mentioned, that no classification of membranes yet proposed by the many followers of Bichat, has been generally received, or has obtained any but partial assent. Inquirers are divided even in opinion respecting the nature of particular struc-

tures; as for instance, that of the middle coat of arteries,—some considering it analogous in nature to the substance of the muscles over which the will exercises no control, and in which the peculiar arrangement or appearance of fibres observable in the voluntary muscles under the microscope is not distinguishable: others, holding it to be totally different from muscular substance, neither irritable nor contractile, but elastic, and most analogous to elastic ligament, as the *ligamentum nuchæ*. This difference of opinion has arisen out of various research, not carried sufficiently far; and is a good example of the state of our knowledge of the subject of membranes generally, in which a no small number of points are open to question, and comparatively few are fixed. Another example presents itself in mucous membranes, viewed in a different relation. It is yet undecided, whether or no the secretion from which they derive their principal characteristic and their name, is solely the production of glandules or follicles dissiminated through their texture, or that of the submucous cellular tissue; or in part that of the membranes themselves independent of such follicles or glandules.

In a former section, namely, on the effects of boiling water, and of long continued boiling on animal textures, I have expressed the opinion, that the results detailed relative to the white membranes and fibrous parts, seemed to me rather in favour of Haller's early view of them, than of the later and

more elaborate one of Bichat,—not that they are all distinct tissues, each *sui generis*,—but that they are analogous in composition, and differ rather in degree than in kind, and that their basis is cellular tissue. When their properties are considered, traced, as it were in transition; and when it is considered how easily animal, and vegetable substances are modified, there is little difficulty in arriving at this conclusion. Starch, it would appear, by some change of molecular arrangement, its chemical composition remaining unaltered, becomes sugar; the change, as it is well known, is produced artificially by boiling it in water acidulated with sulphuric acid. Tendon by simple coction is converted into gelatine; and all the other white fibrous and membranous parts by boiling, are more or less similarly changed in their properties, without being altered, as far as has yet been ascertained, in their chemical composition. And, the conclusion, I apprehend is strengthened, by reference to pathology, physiology, and comparative anatomy, and especially the last.

The test of truth is practical usefulness. No systematic arrangements of membranes or tissues yet promulgated, based on the views of Bichat, can claim this proof; or be considered otherwise than hypothetical. The desideratum now appears to be a careful and unbiassed examination of every different animal structure apart as much as possible from hypothetical views, and the application of the inductive method to the results.

XVIII.

OBSERVATIONS ON THE STRUCTURE OF THE DUCTUS COMMUNIS CHOLEDOCHUS; AND ON THE FLOW OF THE BILE.

MANY years ago, when engaged in minutely examining the common biliary duct and the pancreatic duct in the human subject, I observed a peculiarity of structure belonging to the former, and partly shared by the latter,—which I believe, had not before been noticed,—at least, I have not been able to find it described in any of the works of anatomy, which I have consulted expressly with this object in view.

The peculiarity of structure alluded to, may be brought into view, by slitting open the tubes just mentioned and inspecting them under water, after the removal of adhering mucus by washing. Thus prepared, the lower parts of the ductus communis choledochus and of the pancreatic duct, will be seen provided with a kind of valvular apparatus, composed of delicate angular processes or projections of their inner coat,—pointing downwards, as represented in Plate I, Fig. 1.

Since 1827, when my attention was first directed

to this structure, I have examined the ducts in question in a large number of instances : I have invariably found it to exist ; but sometimes more, sometimes less developed. The processes, or valvular projections, commonly first make their appearance (tracing the passage downwards from the liver) a little above the place of junction of the pancreatic duct, and are continued to the termination of the common duct in the intestine. In the pancreatic duct, they are more limited ; they are confined to its mouth, and occasionally to a small portion of the tube adjoining, not more than two lines from its mouth.

Their valvular nature is easily shewn by the introduction of a probe ; directed downwards, towards the intestine, it passes smoothly without obstruction ; but in the contrary direction, towards the liver, it is impeded and stopped, arrested by the minute sacculi, which exist at the base of the processes.

The use of these processes is implied in their valvular nature and situation : whilst they do not stop the descent of the secreted fluids, which it is the office of the ducts to discharge, they are well adapted to prevent the pancreatic juice from ascending either into the gall bladder or the liver, and the bile from flowing or penetrating into the pancreas.

I have been desirous of learning if a similar structure exists in other animals ; but I regret to say, that in no work on comparative anatomy which I have yet consulted, whether systematic or special,

have I found the termination of the biliary duct minutely described, so as to be able to compare it with the human. The few observations, which I had an opportunity of making, seem to show that this duct at and towards its termination in the intestine in animals of different families, is somewhat various. As the varieties tend to confirm the use of the valvular structure in man, I shall briefly notice them.

In the cat, the biliary and pancreatic tubes are very similar to those of man in the manner in which they terminate; and they are similarly provided with valvular processes, and proportionally, even more strongly developed.

In the ox and sheep, the pancreatic duct terminates apart from the common gall-duct. In the gall-duct of the former, the processes are shorter comparatively than those of man, thicker and rounded, and are confined to the margin of its mouth. In that of the sheep, there is hardly a trace of them, even at the mouth of the duct.*

In the horse and elephant, both of which, as it is well known, are destitute of a gall bladder, the terminal structure of the ducts under consideration is very similar. It is distinctly valvular, and in a manner bringing to recollection the *valvulæ conni-*

* The pancreatic duct in man, at least of Europeans, rarely terminates apart in the intestine; in one instance in which I found it so terminating, the valvular structure of the *ductus communis choledochus* was unusually indistinct.

ventes of the spiral intestine of the cartilaginous fishes.*

In the turkey, two or more hepatic ducts enter the intestine direct and apart ; terminating in the form almost of capillary tubes, prominent and flexible with extremely small orifices, in appearance not unlike the membranous terminal portion of the urethra of the ram projecting from the corpus spongiosum, they are without a valvular structure.

In the ray (*raia batis*) the structure of the biliary duct, where it passes obliquely through the coats of the intestine, near the pyloric end of the stomach, is distinctly valvular, exhibiting transverse folds or plates, offering resistance backwards only contrary to the course of the bile.

Besides the use assigned to the valvular processes in man, to prevent the reflux of the fluids which the biliary and pancreatic ducts are designed to discharge, they may be auxiliary also, towards two other objects, viz. in offering an impediment to the contents of the duodenum entering the common duct,—and in retarding the flow of the bile from the liver into the intestine, and so promoting at certain times its passing into the gall bladder.

* In the horse the hepatic and pancreatic duct terminate in a sinus or pouch-like cavity, which may be considered as belonging to the intestine,—not unlike in its conformation the cavity of the caput galinaginis of the human urethra. In the elephant their termination is very similar : the mouth of the sinus is circular and sufficiently large to admit the end of the little finger ; the cavity is cup-like within.

In the examination of bodies, I am not aware that chyme has ever been found in the ductus communis choledochus ; I have never seen it there. And from what I have observed, I am disposed to believe, that the bile, after death, rarely if ever descends into the intestine, but is retained in the gall bladder and ducts above the entrance of the common duct between the coats of the intestine. This I infer from finding the penetrating portion of duct, not stained with bile, like the portion above, and therefore it may be presumed not containing bile.* I mention these circumstances in proof that the structure of the terminal portion of the duct is such as to act in the manner above-mentioned. Besides the valvular processes described, several circumstances without doubt are involved in the operation,—as the common duct, previous to entering the intestine being enveloped in, or attached to a portion of the pancreas ; its oblique passage through the coats of the intestine ; the diminution of its diameter there ; and its clitoris-like papilla, protected by a fold of the villous coat of the duodenum, somewhat resembling a prepuce, as has been pointed out and described by Morgagni :† and in relation to the gall bladder, in addition to some of these circumstances, its position, partly protected

* This portion of the common duct, may be considered in function analogous to the rectum, commonly empty and collapsed, and distended only just before, and in the act of evacuation.

† Epist. Anat. p. 33.

from the pressure of the abdominal muscles by the margin of the liver ; and perhaps, the relative position of the common duct, having close to it on one side the hepatic artery, and beneath it the vena portæ : and perhaps also, the somewhat spiral and lamellar structure of the cystic duct.*

* The biliary ducts exhibit in different subjects, several varieties,—indeed they are rarely perfectly uniform, either in dimensions, or the manner in which they are connected, and especially the common duct with the pancreas. In illustration of some of the circumstances mentioned in the text, I shall give the results of a careful admeasurement of one ductus communis choledochus,—from a man, aged 30, who died of pulmonary consumption. Slit open, the greatest width of the tube, at the junction of the hepatic and cystic ducts, was $\frac{76}{100}$ of an inch, its least near its termination $\frac{30}{100}$; its length, measuring from the junction of the tubes, was just three inches ; rather more than one-third of its length, viz. $1\frac{6}{100}$ inch, was hid, enveloped in the pancreas and penetrating the coats of the intestine ; where it first joined the pancreas, its width was $\frac{42}{100}$; the length of the portion penetrating obliquely the intestine was $\frac{40}{100}$; and the longest valvular filament or process was $\frac{18}{100}$. In this duct, the valvular apparatus was strongly developed, and when the processes were distended (as they were by immersion in dilute sulphurous acid)—their points crossed obliquely, completely obstructing the passage. Glisson in his *Anatomia Hepatis*, mentions, or rather infers the existence of, circular fibres, in the oblique part of the duct, to which he attributes in part the prevention of regurgitation, “*regressu omnis in ductum communem præpeditur à fibris annularibus, quæ non modo orificium ipsum, sed et totum obliquum tractum obsident.*”† In the duct, just noticed, there was a distinct appearance of transverse fibres, but to a very limited extent, close to the mouth of the pancreatic duct ; they had a silky lustre, not unlike the fibre of tendon or of the dura mater, and their use it may be conjectured, is to prevent undue extension of the part.

† Anat. Hepat. Cap. XVI.

I shall conclude with a notice of some experiments which I made in 1829 on the dead body, connected with the points under consideration :—

Experiment 1st.

Applied a ligature to the pylorus, and made a small opening in the hepatic duct, just above the junction of the cystic, and successively distended by means of a powerful syringe the duodenum with water and with air : neither passed into the duct.

Experiment 2d.

After the duodenum had been well washed out, it was filled to distension with clear water. Pressure was made on the gall bladder and ducts ; the water in the duodenum was found strongly colored with bile ; and bile was found to pass when similar pressure was used, the duodenum being distended with air.

Experiment 3d.

Laid open the duodenum, so as to expose the papilla of the ductus communis choledochus, and having tied the hepatic duct, made pressure on the gall bladder ;—the common duct was presently distended with bile, but no bile passed into the intestine ; a considerable degree of pressure was required to make it flow into the intestine. And the result was the same, and even more strongly marked, when the hepatic duct was not tied.

Experiment 4th.

After having laid open the duodenum, and emptied the gall bladder, by a small opening in its fundus, which was closed by ligature, injected water *gently* through the hepatic duct, it rose more freely in the gall bladder than it passed into the duodenum.

Experiment 5th.

Placed the body in such a position that the gall bladder was raised above the level of the mouth of the common duct, and injected water as before. Now it flowed into the duodenum, but did not enter the gall bladder.

Experiment 6th.

The body lying on its back, injected water forcibly, and with various degrees of force ; it flowed freely into the intestine, and rose in the gall bladder so as to fill it nearly one-third full,—and to the same extent whatever was the degree of force employed ; and which appeared to be (as well as could be judged by the eye) the level of the mouth of the duct, supposing a horizontal line to be drawn from it to the gall-bladder.

Whether there is any vital, peculiar force regulating the expansion and contraction of the mouth of the common duct, and consequently the flow of the bile, it is difficult to determine. That mechanical causes must operate powerfully can hardly be doubted:—the results which I have described seem

to prove it ; and such causes in conjunction with the variable secretion of the biliary fluid, according to the degree of activity of the secerning organ, will probably serve to account for most of the phenomena belonging to the course of the bile both in health and disease.

XIX.

ON THE MALE ORGANS OF SOME OF THE CARTILAGINOUS FISHES.

AT page 56 of the 1st Volume, in the section on the foetal developement of the torpedo, I have briefly described the male generative organs of this fish, as consisting of two firm oval testes, of vasa deferentia without vesiculæ seminales, and of a papilla opening into the intestine near the anus, the common termination of the seminal and urinary passages. On referring to Dr. Müller's able work, "*De Glandularum secernentium Structura penitiori*," in which he treats of the testes of the rays and sharks, I find that his descriptions and views of these organs are not in accordance with the above. His words are: "*Maxime singularis est genitalium masculorum in Rajis et Squalis conformatio; sunt enim organa glandulosa duplicis generis, altera, quæ huc usque tanquam testiculi descripta sunt, ex globulis, non vero ex ductibus seminalibus conflata, altera, plerumque pro epididymidibus habita, ex canalibus*

serpentinis composita, sed minime cum testiculis globulosis conjuncta, quare non epididymides sed glandulas proprias esse conjicio.”*

He adds “organum alterum recte ab Ill. Cuviero jam descriptum est. Dicit enim Cuvierus: ‘ils sont grands, alongés, quoique larges et plats, et s’étendent sous l’épine au-dessus du canal intestinal et de l’estomac. Leur plus grande partie est une agglomération de tubercles de la grosseur d’un pois pressés les uns contre les autres, et présentant chacun un petit enfoncement au milieu de leur face externe. Ils tiennent ensemble par des filaments très-forts, et par la membrane extrêmement délicate, qui les enveloppe, et ne paroissent composés que d’un grand nombre de petit grains ronds très fins. L’autre partie de ces testicules singuliers, est formée d’une substance glanduleuse homogène, qui en occupe, en arrière, la portion la plus mince, et s’étend sous toute la face inferieure de la portion tuberculeuse.’ ” †

After alluding to the similar observations of Treviranus on the same organs in the *squalus acanthias*, and mentioning in confirmation those which he himself made in examining the ray, he comes to the conclusion, that the bodies which hitherto in those fishes had been called epididymides, having no connection with the testes, at least yet discovered, are

* Op. cit. p. 105.

† Leçons d'Anat. Comp. tom. v. p. 27.

glands of a peculiar kind; and he conjectures, (stating, however, that it requires confirmation), that the globular organs are the true testes, and that the spermatic fluid secreted by them, as in the instance of the petromyzon and the eel, instead of flowing through an appropriate canal, may burst into the cavity of the abdomen, and be discharged by the abdominal apertures. *

Such high authority necessarily led me to doubt the correctness of my own observations, and to form the wish to repeat and extend them. This I have been able to accomplish, and I shall now detail

* The passage containing the above conclusion is the following: "Jam vero nunc evictum credo, epididymidem sic dictam, cujus conjunctionem cum testiculis, nemo hactenus vidit, omnes potius supposuere, re vera glandulam proprii generis est. Quaeritur nunc utrum organorum testiculus sit, ac si semen in globuloso testiculo paratur, quomodo semen excernatur. Posset aliquis seminis secretionem in organo altero ex canalibus composito ponere. Sed organon globulosum tam peculiare magnumque est, ut cum alio quopiam organo comparari nequeat. Tum vero meminimus structuram in Anguillis et Petromyzonibus testiculorum globulosam, in quibus semen non per ductum proprium evehitur, sed, uti ova, in abdomen defluens, per orificium simplex excernitur. Quaero, num etiam in Rajis et Squalis semen ex globulis in cavum abdominis propullulet et orificiis illis evehatur, quæ tam in masculis quam feminis Squalis et Rajis obveniunt? licet in feminis ova proprio duplici oviducta evehantur. Hoc observationes ultiores evincere debent. Incertum etiam manet, cujus naturæ sit secretio alterius permagnæ glandulæ, utrum maximum momentum in hocce potius organo positum sit, an glandula testiculis succenturiata sit. Certe liquor glandulæ copiosissimus alius longe naturæ est ac testiculorum globulosorum et a glandula illa ipsa secernitur." He immediately adds: "Itaque vera epididymis in piscibus non adest, in amphibiis et avibus adhuc dubia."

the results, the majority of which have already appeared in a paper communicated to the Royal Society, and which has been published in the 1st part of the Philosophical Transactions for the present year.

1.—*Of the Male Organs of the Torpedo.*

Two good specimens of this fish, from the Mediterranean, of middle size, one an oculata (*T. oculata*), the other a tremola (*T. diversicolor*), belonging to the Museum of Natural History at Fort Pitt, have enabled me to institute a careful examination of the organs in question of these species. The result has accorded perfectly with my first observations made at Malta on the same species in a fresh state.

The testes appeared as I have briefly described them, in relation to form, situation and consistence; exhibiting indistinctly the globular structure described by Baron Cuvier in the passage quoted,*—(*vide Plate 1. Fig. 2*). In neither instance could I discover any traces of the attached soft glandular structure noticed by Cuvier. The epididymis of each organ was comparatively large, and distinctly tubular, terminating below in a large tortuous vas deferens, which pro-

* The form of the testes in different specimens of the torpedo, and of the same species, varies very much; the oval is perhaps the most common; in three now before me, one is of this shape, another is somewhat spherical, and a third is nearly disc-shaped.

ceded to the papilla or rudimentary penis within the verge of the anus ; — and, above, was connected laterally with its testis by a small number of vasa efferentia—using the term hypothetically.

To this connection, of course, my attention was particularly directed. The appearances were most satisfactory : small tubes or ducts could be clearly seen passing from one to the other, and entering into the body of each. They were made perfectly apparent by minute dissection under water, and the immersion of the organs in dilute sulphurous acid,—*vide Fig. 2, (c) Plate I.*

2.—*Of the Male Organs of the Thornback (Raia clavata).*

On the 12th of October I had an opportunity under very favourable circumstances, of examining a male thornback of large size, shortly after being caught. This being the breeding season of this ray, its generative organs were fully developed, and every part of them was peculiarly distinct and large,—as the globular organs, considered by Professor Müller as testes, their soft milt-like] appendage, attached to their inferior extremity and partially bordering their inner margin; the massive epididymis, of tubular structure, conjectured by Professor Müller to be a gland of a peculiar kind, and the vasa deferentia of capacious dimensions, terminating in a kind of urethra, close to the necks of the two sacs,

which I believe perform the double function of urinary bladders and of vesiculæ seminales.

The structure of the milt-like appendage, of the globular testis, and of the epididymis, corresponded to Baron Cuvier's and Professor Müller's description of them. In neither of the two former could I observe any appearance of tubular structure; which was very strongly marked in the last.

As in the instance of the torpedo, I made careful search after a connexion between the testis and epididymis; and using nearly the same means, I was able to satisfy myself that such a connexion exists, and in the same situation, viz. between the superior extremities of the two parts, where the space separating them is inconsiderable (*vide Plate 2*). The tubes of connection, however, were smaller and more difficult of demonstration than the analogous ones of the torpedo.

As the vasa deferentia were distended with a cream-like fluid, which had very much the appearance of spermatic fluid,—it appeared probable, that some satisfactory evidence might be obtained by instituting an examination of the contents of the different parts, and that it would be best effected by means of the microscope.

Using a high power of one-eighth inch focal distance I first examined the fluid contained in the sacs, which perform I suppose the double function of urinary bladders and of vesiculæ seminales. It

was of the appearance and nearly of the consistence of cream. Under the microscope, it was found to abound in animalcules, in active motion, mixed with globules of different sizes. They were best seen when the fluid was diluted with a solution of common salt. The animalcules were proportionally of great length, not unlike portions of fine hair; one extremity was of extreme fineness and seen with difficulty. Their motion was serpentine and vibratory, and when most active, extremely quick, especially the tapering part.

Next I examined the fluid from the vas deferens, taken from its commencement, just after leaving the body of the epididymis. Its appearance to the naked eye and its character under the microscope were very similar to those of the preceding. It abounded in the same animalcules, also in active motion, many of them grouped together in bundles, and which so joined side by side, acted together;—the tapering part, in which the approximation was greatest, moving with great velocity, in a vibratory manner.

Lastly, I examined the fluid yielded by the globular testis, procured by making an incision into its substance, and gently scraping the cut surface. The small quantity of fluid thus obtained was opaque, but not so thick as the preceding. Under the microscope, it was found to contain animalcules similar to those before-mentioned, and in motion; but less

numerous and not grouped ; intermixed with small globular masses of an obscure granular and radiated structure.

3.—*Of the Male Organs of the common Skate (Raia batis).*

On the 8th of November under the same favourable circumstances, I examined a male fish of the above species, in which the generative organs were fully developed, and which generally in no respect essentially differed so far as I could observe from those of the thornback.

In this instance, for the sake of as much accuracy as possible, I began with an inspection of the fluids, likely to throw light on the functions of the generative organs.

The fluid first subjected to the microscope was some contained in the cavity of the abdomen. It was transparent with a small opaque sediment ; and was found to contain globules of different sizes, the largest less than common pus-globules, and a few elliptical blood particles, without any animalcules.

The fluid next collected was that contained in the urinary bladders. It was nearly colourless and limpid. Under the microscope, many small globules were visible in it, about one-half the size of pus-globules and a few animalcules resembling those of the thornback, and yet not precisely similar.

On pressing the vasa deferentia, where they pass

on the inner side of each bladder, a cream-like fluid was discharged into the cloaca through the papilla, —the termination of the urinary, and, as I believe, seminal ducts. This fluid under the microscope was found to abound in animalcules, mixed with a few globules, both similar to those last-mentioned ; the former in active motion.

Next, the vas deferens was opened into, before it passes behind the urinary bladder, and some fluid was obtained from it, not inconsiderable in quantity, and of the colour nearly and consistence of cream. Under the microscope, it was found to abound in animalcules, of a thread-like form, having one extremity excessively fine, very active in movement ; their motion vibratory, as well as progressive ; in every respect closely resembling those of the thornback ; and like them, —owing to their vast number, and being intermixed with many globules, to be seen distinctly, the fluid required to be diluted.

The globular testis was next cut into ; a portion of it was removed by a horizontal incision. It abounded in fluid, more liquid than that of the vas deferens, and less opaque. Under the microscope, many animalcules were seen in it, precisely similar to those of the vas deferens,—but not in motion, as if dead ; and mixed with them were many globules of different sizes, the largest about the size of pus-globules, a few blood corpuscles and some fragments of irregular form.

Lastly, an incision was made into the lower non-

globular, milt-like part bordering the globular testis. It abounded in thick opaque fluid, of which a sufficient quantity was collected for examination by gentle pressure. Under the microscope, it was found to contain a large number of globules, of about the size and general appearance of pus-globules, a few very much smaller, and a few animalcules, less distinctly formed than those in the globular portion, and in the vas deferens, but clearly of the like kind.

The anatomical examination of the organs was next entered on. I have stated that at first view they appeared generally not to differ from those of the thornback; a minute inspection confirmed this. A tubular connexion was found between the head of each testis and epididymis, not admitting of doubt; the tubuli were traced from the globular substance into the mass of the epididymis.

On the 24th of November, another fish of this species was procured, in which also the male organs were in a very favourable state for examination, and which were examined with great care, having in view the doubtful points.

No fluid in this instance was contained in the abdominal cavity; not a single drop could be collected. As the globular and the milt-like testis, both of this ray and of the thornback, are connected with the epididymis, by a delicate peritoneal covering,—enveloping the whole of them, leaving a cavity between the testis and epididymis, which descends

along the spine close to the bladder,—it occurred to me, as possible, although not probable, that this cavity might be a channel of communication between the testis and the cloaca. To endeavour to determine this, a small opening was made into the cavity, and it was forcibly distended with air by means of the blow-pipe; but no orifice inferiorly could be detected; the air was completely confined.

This done,—the fluid contents of the different parts were subjected to microscopical examination, in the following order,—first, that of the urinary bladder; secondly, that of the milt-like testis; thirdly, that of the globular testis; and lastly, that of the vas deferens. The results in part were somewhat different. The fluid contained in the two urinary bladders amounted to four-fifths of a cubic inch; like the former, it was colourless and transparent with a very slight sediment; it contained a few globules, but no animalcules. The milt-like part of the testis yielded but little fluid: a minute quantity of it collected by scraping an incised surface, under the microscope exhibited no animalcules,—but many well defined globular particles of different sizes commonly smaller than pus-globules, and a few blood-corpuscles. The globular portion of the testis abounded in fluid, of a creamy appearance, containing many animalcules, mixed with globular particles; the former precisely similar to those observed in the first instance, and like them motionless. The fluid of the vasa deferentia, which flowed

out on opening into them, amounted to seventenths of a cubic inch. It had the same character as that procured from the canal in the other specimen, and abounded in animalcules in every respect similar, and like them in active motion.

The anatomical examination in this instance afforded a result precisely similar to the preceding; a connection by means of tubuli was discovered between the head of the testis, and of the epididymis; and no where else,—after very careful search. The whole of the generative organs with the urinary, associated with them, were removed entire;—and were immediately examined under spirit of wine.

4.—*Of the Male Organs of the Spotted Ray,*
(*R. Maculata.*)

On the 9th of February, through the kindness of Dr. Andrew Smith, who procured the fish,—I examined a specimen of this species about two feet long, which had been out of water about 48 hours. The generative organs appeared to be fully developed, and in their character similar to that of the common ray and thornback; the milt-like, or, as Monro calls it, the medullary part,* however, not only bordered the inner margin, and was appended to the inferior extremity of the globular testis, but also spread over a considerable portion of its pos-

* On the Structure and Physiology of Fishes, Edin. 1785, p. 86.

terior surface. There appeared to be a connexion between the upper portion of the epididymis and of the testis by three or four filaments or tubes.

These in the first instance I carefully cut out, avoiding, with the scissors, equally the testis and epididymis. It was agitated in a little solution of salt, and then subjected to the microscope: many animalcules were now seen in the solution and globules of different sizes. The animalcules were without motion; in form they closely resembled those already described. And similar animalcules were found in the fluid of the globular testis, with like globules, and also a bundle of animalcules. In the milt-like appendage, globules only were observable, of different sizes, the largest appeared as if composed of very many extremely small ones. The entozoa were found in abundance in the cream-like fluid of the epididymis, — which in no wise differed in appearance from those of the testis and connecting tubes or vasa efferentia.

5.—*Of the Male Organs of Scyllium Edwardii.*

This is the only species of shark the male organs of which I have recently had an opportunity of examining: it was brought by Dr. Smith, from the Cape of Good Hope, and to this gentleman I am indebted for permission to inspect it. The parts were not in the best condition; they had suffered from keeping, especially the testes. The epididy-

mides were large, as were also the vasa deferentia, which terminated, as in the foregoing instances of the ray, in a kind of urethra, connecting the urinary bladder with the cloaca.

Having found that the spermatic animalcules of the mammalia, are but little liable to change,—that they may be detected even in a putrescent fluid, and may be kept for many months, and probably years in spirit of wine,—I thought it worth while to examine, with the microscope, any fluid that might be found in the vasa deferentia of this scyllium: a little turbid fluid mixed with grumous matter was procured by laying them open, in which when diluted, animalcules were distinctly seen, resembling much those of the rays.

This result induced me to try the contents of the testes, and of the vasa deferentia of the torpedo. The experiments were made with two fishes, varieties of the tremola, rather below the middle size, which I had sent home from Malta, more than four years before, preserved in spirits. The vasa deferentia of both were large, and when opened, afforded pretty much opaque white semifluid matter. When diluted with water, and placed under the microscope, animalcules were distinctly visible in it; thread-like, serpentine, and finely tapering towards one extremity, very similar to those of the common ray and thornback, but decidedly smaller. And animalcules of precisely the same kind, but less numerous, were detected in the semifluid opaque matter in

minute quantity obtained by scraping gently the cut surface of the testis.

6.—*Of the Accessory Male Organs.*

Before entering on the inferences to be drawn from the foregoing observations relative to the functions of the different parts constituting the male organs which are contained within the abdominal cavity, I would wish to offer a few remarks on the external accessory organs, which have commonly been considered auxiliary to the more important internal ones. They are the anal appendages, which are characteristic of the male cartilaginous fishes,—organs of complicated and curious structure, the use of which at present is far from being understood.

The torpedo, the common ray, and the thornback, are the only species of ray which I have yet carefully examined in relation to the organization of these parts. In each species they are very similar, consisting of articulated bones, muscles, mucous ducts, &c. and containing a large and remarkable gland associated with an elaborate and complicated structure.

On account of the large size of the common ray, and its large anal appendages and their full development,—the gland and its accompaniments are seen in this fish to great advantage. In two specimens of *Raja batis*, which I have examined, each

about three feet long, the gland was nearly the size of a chestnut, (vide Pl. iii. Fig. 1.) of a very elongated oval form, divided on one side, as it were into two columns, by a superficial furrow or depression, in which were two rows of delicate projecting tubuli, the extremities of its excretory ducts.

The substance of the gland was enveloped in a muscular coat, and this was covered with a vascular tissue. The gland itself was contained in a sac, composed of three coats—an inner fibrous, a middle muscular, and an outer cellular one; and was surrounded with strong muscles—the principal flexor and extensor muscles of the organ.*

Moreover, at the inferior extremity of the sac, just below its outlet, was a distinct cavity formed of muscular walls, and intersected by delicate tendinous fibres. (Vide Fig. 2. Pl. iii.) In one instance, when under examination, the fish was still irritable, its muscles acting when stimulated; and then this part pulsated regularly and vigorously. It contained blood; and I believe it to be an auxiliary heart designed for circulating the blood in the appended

* Bloch, in his description of these organs, describes only two muscles; but there are more; some connecting the organ with the pelvis, others attached to the principal bones, and others to the small bones: which is as might be expected, —considering that eleven bones and a terminal cartilage enter into the composition of each organ. This number I found in the instance of the thornback; Bloch states, that in this fish they are eight. *Hist. Nat. des Poissons*, iii. 672.

organ. A similar structure exists in the same situation in the thornback and torpedo.

In the sac of the gland a cream-like secretion was found, and the same flowed out pretty copiously through the excretory ducts when pressure was applied to the gland. This fluid was neither acid nor alkaline; it was slightly viscid; applied to the tongue, no sensation was immediately produced; perhaps there was an after one approaching to acrid, but so slight as to be doubtful. Under the microscope, it was found to abound in very minute dense spherical particles, twenty of which at least would be required to form a mass equal to a blood corpuscle of man. They had no appearance of independent vitality, and moved only when in currents. The blood in the pulsating cavity, from which it is probable that the secretion just mentioned is formed, coagulated like ordinary blood on exposure to the air; but it was more dilute, and, what is remarkable, under the microscope, its particles appeared to be smaller, and the majority of them not elliptical but globular.

How the anal appendages are constituted in sharks, I cannot speak from my own observations, having yet examined these organs only in one instance,—that of *Scyllium Edwardii*, before referred to. From the descriptions of naturalists, it may be inferred that they vary more or less in organization, in different genera,—that in some, as probably in the genus *Carcharias*, there is a distinct gland, se-

creting an opaque fluid, similar to that of the rays I have mentioned; that in others, as in the genus *Scyllium*, the gland is wanting, and its place is supplied by a sac, (one for each organ,) situated under the common integument of the lower part of the abdomen, communicating by a narrow elongated passage, with the appendage; and containing a fluid slightly viscid,* probably secreted by follicles situated between the fibrous inner coat and its outer muscular one. This structure, I am informed by Dr. Andrew Smith, who, it would appear, first observed it, occurs in every species of the genus; and as in one specimen, that of the *Scyllium Edwardii*, the gland is not to be found, (we carefully sought for it together,) it is probably deficient in the others.

7.—*Concluding Remarks.*

As in the torpedo, the common ray, the thorn-back, and the spotted ray, a tubular communication has been found to exist between the globular testes and the bodies hitherto called epididymides; as the fluid in the former was found to be similar to that of those canals—continuations of the epididymides which have commonly been considered as vasa deferentia; and as this fluid in its entozoa, possesses the essential character of a spermatic fluid; and lastly, as it could not be detected free in the cavity

* According to Dr. Smith, the fluid contents are like very dilute white of egg.

of the abdomen, and in the only instance examined, that of the spotted ray, it was actually detected in the connecting tubes; may it not be inferred, and is not the conclusion unavoidable, that the old opinion respecting the functions of these different parts is correct, and in accordance with the names which they have received?

The evidence, on which this conclusion is founded, is manifestly of two kinds; one anatomical; the other microscopical, connected with the constitution of the spermatic fluid. Preparations have been made of the parts shewing the tubular connexion, which are deposited in the collection of comparative anatomy belonging to the Museum at Fort Pitt. Relative to the evidence derived from the nature of the spermatic fluid, I apprehend, now, it will generally be admitted as satisfactory. I have examined the spermatic fluid of many species of mammalia, and the animalcules contained in it were in no instance more distinct than those of these fishes; indeed in the latter they were perfectly so, and their character well marked. They were immediately killed by spirit of wine; they were torpid and motionless in a saturated solution of common salt; they became active when the solution was diluted;* and in the

* The effect of fresh and of salt water on the spermatic entozoa of fishes which breed in the sea and in rivers and lakes, will probably be found to be opposite. This conjecture, I found on the observations mentioned above, of the different effects of fresh and salt water on the animalcules of the rays, in connexion with the additional one, that the spermatic animalcules of the milt of the salmon

unmixed fluid they retained life for three or four days after removal from the fish,—until signs appeared of incipient putrefaction.

The testes of the torpedo are distinguished from those of the thornback and common ray, by wanting that milt-like margin or appendage, described by Baron Cuvier, and before noticed and figured by Monro.* Should it be found wanting in all the viviparous species of cartilaginous fishes, its presence in the oviparous may be considered as a link between them and the osseous oviparous fishes furnished with milts. Perhaps in the milt-like part of the testes of the ray and thornback, the ova of the

are not excited into motion by salt water, but by fresh water. Should the conjecture I have thrown out be confirmed by more extensive observation, we shall have reason to admire so much the more the refined adaptation of that instinct by which certain fishes (as the salmon) are impelled to quit the sea their native element, and ascend rivers, the fresh water of which, whilst it is injurious to their own health, is probably alone fitted for the early development and life of their young. The sensibility of young fishes relative to the quality of water appears to be very great; the foetal torpedo dies more rapidly when plunged into fresh water than when exposed to the atmosphere; in the former in about fifteen minutes it dies convulsed.

* *Op. et loc. cit.*—Dr. Monro, in his able work on Fishes, describes the organ which he considered as holding the place of the testicle, as “a substance composed partly of a white matter like the milt, and partly of small spherical bodies.” He adds, “from these an epididymis is produced, chiefly composed of convoluted tubes, which terminate in a serpentine vas deferens.” His attention does not appear to have been particularly directed to the connexion between the testis and the epididymis; as he makes no mention of the difficulty of demonstrating it, it may be inferred, that he took its existence for granted.

spermatic animalcules are developed ; and perhaps in the globular portion they grow and are matured. The microscopical observations described are rather in favour of this idea.*

If the view which I have taken of the function of the testes and of the epididymides be received, the structure of the subordinate generative parts within the cavity of the abdomen will, I believe, be found to be perfectly in accordance and to offer fresh evidence in favour of its correctness : I now allude to the vasa deferentia,—and the part or parts inferiorly connected with them.

The vasa deferentia, in all the rays which have been noticed, are similar, and well adapted both to conduct and hold in reserve such a fluid as the spermatic,—for they are comparatively large, and are

* The spermatic fluid of the milt of the bony fishes, bears some resemblance to the fluid of the milt-like portion of the testes of the rays mentioned. I have examined it in the herring, smelt, cod, dab, pike, and salmon; in the three first, it appeared minutely globular; in the dab, many of the particles had two opposite delicate filaments, and in the pike one.

MM. Dumas and Prevost state that the spermatic animalcules of all the fishes they had examined are filamentous,—and that the filamentous part had escaped observation previously on account of its great tenuity; an opinion I can readily adopt, as I could perceive the prolongations of the animalcules of the dab and pike, only when in a very favourable light, and by means of nice adjustment. What the species of fishes were, examined by these gentlemen, is not noticed; their remark on the subject is made incidentally in their ingenious memoir on the spermatic animalcules of many of the vertebrata already quoted, with the promise to give the details on a future occasion. *Ann. des Scien. Nat. tom. i. et ii.*

provided with circular *valvulae conniventes*, forming a vast number of cells. Both in the ray and thornback, and torpedo, they terminate, in what may be considered the urethra of a rudimentary penis,—the end of which projects into the cloaca in the form of a papilla. In the torpedo, which differs from the other two in having no urinary bladders, the spermatic and urinary ducts terminate near the mouth of the papilla, by separate and very minute orifices. In the thornback and ray, which are possessed of two urinary bladders, these ducts terminate near the fundus of the short urethra, and close to the neck of each bladder, or its opening into the common passage.* These bladders appear to be somewhat analogous to the bifid bladder of frogs and toads, the latter connecting them with the single urinary receptacle of the higher vertebrata.

I have expressed the opinion that these organs may perform the double function of *vesiculae seminales*, and of urinary bladders, partly from their situation and connexions, and partly from their contents. I have mentioned that in one instance I

* Delicate probes passed through the bladder, ureter and vas deferens of the common ray and thornback, meet at the bottom of the short urethra—common to both sides; the openings into it are small and contracted; the vas deferens on each side terminates in it by a projecting papilla, having an oblique direction. I have carefully sought for a termination of these ducts into the bladder itself,—but in vain. I mention this particularly, because a passage in the valuable Descriptive Catalogue of the Museum of the Royal College of Surgeons, would seem to imply such a termination. (Vol. iv. p. 51, No. 2394).

found in them some spermatic animalcules, which may be considered equivalent to spermatic fluid. The result of a chemical examination of one specimen of the fluid which was contained in them,—in the instance of the common ray, amounting nearly to a cubic inch, was favourable to its urinary character; for besides affording a little saline matter, principally common salt, it yielded a little animal matter very analogous to urea, soluble in alcohol, uniting with nitric acid, and the compound crystallizable and soluble.

The nature of the anal appendages and their functions have, from the earliest times of natural history, been more or less a subject of controversy. Aristotle considered them as characteristic of and peculiar to the male cartilaginous fishes. Lorenzini erroneously denied that they were distinctive marks of the sexes. Willoughby, Ray, Artedi, and Broussonet considered them as organs of intromission, as penes. Rondelet opposed this notion, and considered them as holders, in which opinion he was followed by Bloch, who I believe was the first to describe them with tolerable accuracy and minuteness.* Recently, some naturalists appear to have adopted one conclusion, some the other; the majority favour the idea of Bloch,—that they are analogous, as he ingeniously endeavoured to prove, to arms, or rather feet, intended for seizure and holding fast. A small

* His description was confined to these organs in the thornback: *Hist. Nat. des Poissons*, iii, p. 672.

number, amongst whom M. De Blainville is eminent, seem to have returned to the older notion that they are penes. Bloch's argument against their being penes is founded on their structure, in his opinion, totally unfit for their supposed office. Those who maintain the opinion he opposes, lay stress on the appearance of the fluid secreted by the gland belonging to each appendage, so much resembling the spermatic fluid, and on the analogy in certain of the reptiles of a double oviduct and double penis. Objections, it appears to me, are unavoidable to both of these views;—as regards the latter, it is highly improbable, that there should be two sources of spermatic fluid; in fact that there should be four testes: moreover it has appeared, that the fluid of the appendages is without the characteristic quality of the spermatic fluid, containing no entozoa, and seeming to be of a peculiar kind. As regards Bloch's view, it seems improbable, were the appendages designed for prehension, that they should be furnished with a gland, copiously secreting a lubricating fluid:—nor does their general structure or situation appear to be apposite for the purpose he imagines, especially as he is of opinion that the papilla of the male, its rudimentary penis, is applied in the act of impregnating to the surface of the cloaca, where the mouths of the oviduct or uterine cavities open.

Reflecting on the subject, and on the inadequacy of former hypotheses, it has occurred to me, as pos-

sible, that these organs may be designed for intromission, and for retension like the penis of the dog; on this idea, the abundant secretion of the gland as a lubricating fluid would be of manifest use; and the action of the different parts might admit of explanation.* And, as the sacs connected with the appendages in some of the sharks open appropriately, they may be supposed to be designed for the same use.† In venturing to bring forward this conjecture, I beg to be understood, that I attach no kind of importance to it. I am fully aware that it is liable to objection, or at least that it wants proof. It seems, however, deserving of consideration; and in offering it to notice, I shall be satisfied, should it excite curiosity and lead to farther inquiry, by which alone the true use of these mysterious organs can be determined.

* After writing the above, on referring to Aristotle, that great and curious store-house of Natural History, I found that the same notion had been entertained in his time, and that something of the kind is reported as having been actually witnessed: "Sunt qui se vidisse confirmant nonnulla ex cartilagineis aversa modo canum terrestrium cohærere."—De Hist. Animal. l. v. c. v. Aristotle probably heard this from fishermen, a class of men from whom principally we must expect to derive information respecting the habits of fish, and who, in the clear and often perfectly calm waters of the Mediterranean, and especially of the Archipelago, have the best opportunities for making such observations.

† In the specimen of *Scyllium* which I examined with Dr. Smith, there was an interruption between the outer opening of the sac near the anus, and the slit or groove in the appended organ, so that the fluid poured out, in part, would be directed towards the anus, and in part, if copious, flow over the short intervening space of smooth skin, and enter the groove, and descend to the extremity.

I have mentioned, that most of the preceding observations were contained in a paper presented to the Royal Society: that communication was made in December last year. Since then, I have been informed that Professor Müller has relinquished the views which I have combated respecting the male organs of these fishes, and that he has satisfied himself there is a connexion between their testes and epididymides. This he mentions, in a letter to Dr. Smith, dated Berlin, March 11, 1839, in reply to one, in which the results of my inquiries had been brought to his notice. It appears that it was in examining the torpedo, he first obtained proof of the connecting tubuli, and was led to alter his opinion; and that he announced it, in a note, as long ago as 1836 in his Archives of Anatomy and Physiology. Entirely ignorant as I was of these his later researches,—the two trains of inquiry may be considered quite independent; and as they lead to the same conclusion, they afford strong confirmation to each other. The details of his observations, Professor Müller promises to give in his work on the Myxinoïdes, illustrated with figures, by which there can be no doubt, this distinguished inquirer will lay science under fresh obligations.

XX.

OBSERVATIONS ON PUS.

THE observations which I propose to offer on this subject are of a miscellaneous kind, with the hope of contributing something to advance our knowledge of this very important fluid.

1.—I have made several experiments to endeavour to determine if the pus of an abscess contain air. In each instance, the pus as it flowed from the abscess, carefully opened at the moment, was received into a vial full of distilled water, that had been subjected to the air-pump until entirely deprived of air ; and as soon as it had acquired the temperature of the atmosphere, it was submitted to the action of the pump, the same instrument as before noticed, and in good order.

The first trial was on thick pus from an abscess in the neck of a young man recovering from an attack of erysipelas of the face. The quantity received into the water was small ; it remained at the bottom and was perfectly tranquil ; not a particle of air was

disengaged from it, when the exhaustion was as complete as possible.

The second trial was made on pus from an abscess in the arm of the same patient, opened the day after the preceding; and in this instance also the result was entirely negative.

A third trial was made on pus from the same person, obtained from an abscess which had formed in the axilla, opened three weeks after the last, when his convalescence was well advanced; and again the result was negative, not a particle of air was disengaged from it, in the most perfect vacuum that could be formed.

The fourth trial was on pus from an abscess in the arm of an old soldier, after a very severe attack of erysipelas, (diffuse cellular inflammation,) which for many days had endangered life. The matter in this instance was rather thick, and it did not subside readily in the water. Submitted to the air-pump, three or four bubbles of air rose from it. This occurred, before the vacuum was nearly complete; seeming to indicate, that the air did not belong to the pus, but was accidentally entangled, in passing from the abscess, into the vial; and in confirmation it may be remarked, there was no general effervescence, or separation of air from the pus, even of the slightest kind, when the exhaustion was carried as far as possible.

These trials then tend to prove the absence of air in pus from an abscess. If, however, air in any

way have access to the purulent fluid, and it be subjected to the air-pump, the result, as might be expected, will be different. In one instance, I had proof of this in an experiment with the air-pump on some pus from the pleura, in a case of empyema complicated with pneumathorax, of many months' duration, in which an opening had spontaneously formed in an intercostal space, discharging daily a small quantity of purulent fluid of good consistence and colour; although entirely free from fœtor, this pus under the exhausted receiver gave off a good deal of air.*

* Some pages back I have given the results of experiments apparently shewing, that air is not contained in the animal solids and fluids in their normal state. I perceive, that Professor Burdach has arrived at a different conclusion, and that he refers to Dr. Dalton in support of it. In the 8th volume of his *Physiology considered as a Science of Observation* (cap. ii. 4.) he remarks: "There are many inorganic bodies, especially water and the earths, which absorb air, and sometimes even to an extent exceeding their own volume, and consequently condense and render it latent. The organic substance also exhibits an attraction of this kind for air in a very high degree. Dalton (*Bibliothèque Univ. de Gen. t. liv. p. 130*) has proved in the following manner, the existence of a considerable quantity of air in combination in the human body.

"The lungs and stomach are the only parts of the body which contain free air; the former with a capacity of 100 cubic inches, the latter of 50; but the volume of the whole body is about 4500 cubic inches, or setting aside the lungs and stomach, 4350 cubic inches for the whole of the remaining solids and fluids. Now, as the mean specific gravity of these is 1050, their weight for their volume ought to be equal to 4567 cubic inches of water, that is, 164 lbs.; but the real weight of a man alive of the bulk in question is only 146 lbs. that is equal to 4044 cubic inches of water; consequently it is necessary that the solids and liquids during life should be penetrated

2.—It is commonly stated that the specific gravity of pus is 1030.* According to my experiments this quality is far from fixed; I have found it con-

with air; and that thus they should attain a specific gravity inferior to that which belongs to them when examined apart, detached, after death, when a portion of their air is disengaged."—"Dalton (he continues) states further in support of this argument, that when the hand is applied to the orifice of a receiver on the plate of an air-pump, on exhaustion, it is felt to be drawn in and to swell, because the air contained in the part tends to escape."

Neither the argument used by this distinguished man, nor the example, I apprehend, can with propriety be opposed to the results of actual experiments. But waving this, are the data on which he reasons, established in fact? Is the mean specific gravity of the solids and fluids so high as 1050? And is the specific gravity of a dead part detached, independent of difference of temperature, higher than before it was detached and during life? I believe a sufficient number of accurate experiments have not yet been made to determine the first question. The second I believe must be answered in the negative; from all I have witnessed in trials with the air-pump, I am convinced, that instead of air being expelled after the excision of a part and after death, and the specific gravity of the part increased, the contrary happens,—air enters into it, especially its divided vessels, and its specific gravity is diminished. Even admitting the data, the reasoning seems defective, inasmuch as though the specific gravity of the whole body must be diminished by the penetration of air, the weight of the whole body must be increased by it. Relative to the drawing in of the hand and its apparent swelling, in the instance adduced, the effect admits of easy explanation, by reference to the operation of atmospheric pressure disturbed: on the side from which the air is exhausted, resistance is withdrawn, the hand is pressed in from the force acting from without, and if a blood-vessel be opened, [as is witnessed in the operation of cupping, the vessels being subjected to pressure on one side and not on the other,] the blood will flow freely in vacuum. I have already given the result of subjecting portions of vessels containing blood to exhaustion, not partial but on every side, indicating no expansion, no swelling.

* Dr. Pearson found, in two instances, a certain relation between

siderably variable. I shall state some of the results which I have obtained.

i.—From the pleura, in the case already referred to, of empyema, complicated with pneumathorax, it was of specific gravity 1028; it was pus of good quality and ordinary consistence, “Album, leve et æquale.”

ii.—From an abscess in the thigh, not quite equable, its specific gravity was 1031.

iii.—From the axilla, in convalescence from erysipelas, pretty equable, of moderate consistence; it was of specific gravity 1029.

iv.—From the arm, also in convalescence from erysipelas of a dangerous character, (before alluded to, as well as the last,) its specific gravity was 1036.

v.—From an abscess in the back of a young man, its specific gravity was 1040; it had a slight reddish tinge, and, under the microscope, was found to contain a very few blood corpuscles.

vi.—From a large cavity in the lung in a fatal case of pulmonary consumption, its specific gravity was 1042; it was rather thicker than the healthy pus of an abscess.

vii.—From a vomica in the lung, in another fatal case of pulmonary consumption, it was of specific gravity 1021.

the specific gravity of the pus, and of the serum of the blood of two individuals; in one, that the pus was of specific gravity 1031, the serum was of specific gravity 1029; and in the other in which the former was 1033, the latter was 1031.—Phil. Trans. for 1810, p. 295.

This variableness of specific gravity is probably owing to two circumstances, chiefly,—to variable proportions of the ingredients of the specimens tried, and the variable density of the liquid part. If, *cæteris paribus*, the liquid part of the pus predominates, it will probably be of comparatively low specific gravity; if the concrete part is in excess, the pus-globules, it will probably be of high specific gravity.*

The pus-globules themselves, I believe to be not less heavy than the blood corpuscles; I have come to this conclusion by mixing small portions of pus with solutions of sulphate of soda, of different degrees of strength; with one of specific gravity 1132, the globules were for a time suspended, slowly subsiding rather than rising towards the surface; and, in this instance, when examined by the microscope, the form of the globules did not appear to have undergone any distinct change.†

What the specific gravity of the liquid portion is, I have never ascertained in any example; it is, indeed, a point difficult to determine, unless a large

* Dr. Pearson, in his paper entitled "Observations and Experiments on Pus," in *Philosophical Transactions*, for 1810, notices four kinds of pus.—1, The cream-like and equally consistent; 2, the curdy and unequal in consistence; 3, the serous and thin kind; 4, the thick, viscid, or slimy. He states that he found the specific gravity of each kind very similar.

† Concentrated solution of sulphate of soda imparts viscidness to pus,—but more slowly and in a less degree than muriate of ammonia, and appears to have less effect on the general appearance of the pus-globule.

quantity of purulent fluid can be employed for procuring it; for the concrete part, the globules, and the liquid part, cannot be separated by filtration, owing to the viscid quality of the latter; and owing to the same quality, the globules on rest subside very slowly, and rarely leave the supernatant fluid perfectly transparent; the supernatant fluid, under the microscope, is commonly found to contain globules, the majority of them smaller than the ordinary pus-globules, and of a form less uniformly regular.

3.—As might be expected, pus freezes less readily than water. In the winter of 1811, I exposed some pus from an abscess, to the open air over night, when the temperature was several degrees below 32°. On the following morning, it had an appearance not unlike that of frozen milk, semi-hard, easily broken, composed of large thin plates, with fluid intermixed, arranged vertically. On thawing, the pus did not recover its original properties. Previously it was not viscid, excepting in a very slight degree; now it had become so distinctly, and was slightly ropy. It was also partially coagulated; and it mixed less readily with water.

4.—Certain fluids, mixed with pus, have the effect of rendering it viscid, like mucus. Solution of potash and soda, and ammonia, act thus in a remarkable manner, and a solution of muriate of ammonia and other neutral salts act similarly, but in a less degree. These are facts which have been long known. Two views have been taken of the effect.

It was supposed by Mr. Hunter, who first noticed specially the phenomenon, that it was an instance of coagulation,* an opinion which has been adopted by some recent writers ; thus one, deservedly eminent, (M. Andral) in an article on pus in the *Dictionnaire de Médecine*, noticing different varieties of purulent fluid, remarks,—“ Tous en définitive, paraissent être composés de globules semblables, nageant dans un fluide, lequel est susceptible de se coaguler, lorsqu’ on le mêle à une solution de muriate d’ammonique.” Dr. Pearson, on the other hand, has denied that it is a coagulation, and supposes it to be an inspissation, “seemingly occasioned,” as he says, “by the solution of muriate of ammonia attracting water from the pus.”†

* Mr. Hunter describes pus as a liquid, “composed of very small round bodies, very much like those small globules which, swimming in a fluid, make cream.” He adds, “these globules swim in a fluid, which we should at first suppose to be the serum of the blood, for it coagulates with heat like serum, and most probably is mixed with a small quantity of coagulating lymph; for pus in part coagulates, after having been discharged from the secreting vessels, as mucus is observed to do. But although it is thus far similar to serum, yet it has properties that serum has not. Observing there was a similarity between pus and milk, I tried if the fluid part of pus could be coagulated with the juice of the stomach of other animals, but found it could not. I then tried it with several mixtures, principally with the neutral salts, and found that a solution of sal ammoniac coagulated this fluid: not finding that a solution of this salt coagulated any other of our natural juices, I concluded that globules, swimming in a fluid that was coagulable by this salt, was to be considered as pus, and would be always formed in sores that had no peculiar backwardness to heal.”—(*Treatise on the Blood, &c.*, in vol. iii. of *Works*, p. 449.

† *Philosophical Transactions*, for 1810.

From the observations which I have made, both these modes of explaining the effect appear to be inadmissible. When I had mixed a little of the fluid part of pus, decanted tolerably clear, with a solution of muriate of ammonia, no change was produced, not the slightest degree of viscosity was imparted; but when some of the opaque part containing globules was added, it shortly became viscid. And in confirmation, I may remark, that when viscosity is imparted to pus either by the saline solution just mentioned, or of common salt, or of nitre, the quality is not destroyed by dilution; the greater part of the saline matter may be removed by repeated additions of water, and yet the pus will remain viscid; and further, that if pus be mixed with a large quantity of water in a tall jar, and the water be decanted after the pus-globules have subsided, and this repeatedly till the liquid part of the purulent fluid is entirely removed, the globules collected on a filter become viscid on admixture with a saline solution, almost as readily as if they had not been so treated, and not in an inferior degree.

Dr. Pearson makes a statement apparently contrary to what I have advanced; he says, if the purulent fluid rendered viscid by a strong solution of muriate of ammonia be agitated in cold water, "the matters are diffused, and on standing, the pus is precipitated in its original state;" and also, that viscosity is not imparted by a dilute solution of the salt. In answer to this, I shall notice the appearances I have observed

in making an experiment, to test the value of his remark. A strong solution of muriate of ammonia was diluted with rather more than an equal volume of water, into which, so diluted, a little pus of an abscess was poured, and they were mixed by agitation. The mixture was of a light greyish hue, translucent and equable; the globules shewed no disposition to subside, marking a slight degree of viscosity, and which, on trying it with a probe, it was found to possess. It was now thrown into a large quantity of water, by which its viscid property became very conspicuous. It immediately sunk, and did not mix with the water, keeping together in a mass like the glairy white of an egg, similarly treated, or like mucus, which, under water, it greatly resembled; and the resemblance was more striking from filaments being raised from the general mass, by minute air bubbles disengaged and suspended in the viscid matter.

As the concentrated solution of muriate of ammonia and the other neutral salts, in my experiments, have not rendered the fluid of pus viscid; as they do not render serum viscid, and there appears to be no necessary relation between concentration and viscosity, the conclusion I have come to, and which has been confirmed by all I have witnessed, is, that the effect of viscosity is equally independent of inspissation, and coagulation, and is produced on the globules themselves, which in fact are the part which exhibit it. In accordance with

this, when the viscid fluid is examined with the microscope, its globules have a somewhat fainter appearance, as if their transparency were increased; and they also appear a little expanded, and adhering together more or less, and if moved, when in the field of the microscope, some of them may be seen to be a little elongated.*

6. According to a recent analysis of pus by Dr. Gueterbock, 100 parts of this fluid from an abscess in a mamma, were found to consist of—

86·1	Water.
1·6	Fatty matter, soluble only in hot alcohol.
4·3	Substances soluble in cold alcohol, viz. fatty matter and ozmazome.
7·4	Substances soluble neither in hot nor cold alcohol, viz. albumen, pyina, and the substances of the globules (“globuli et grana puris.”)
0·6	Loss.

100·0 †

From my own experiments I am disposed to consider these results as correct and deserving of confi-

* In accordance with this view, it may be remarked that the time required for different varieties of pus to become viscid on admixture with solution of muriate of ammonia is far from constant; some exhibit the effect almost immediately; others, not till after several hours.

† De Pure et Granulatione, p. 17.—Berol. 1837.

dence. They have been confirmed generally in two or three instances, in which I have submitted pus of an abscess to analysis. The method of analysis which I have followed has been somewhat different from that used by the ingenious author just quoted.

First, The purulent fluid was slowly evaporated to dryness, at a temperature below that of boiling water.

The yellow residue of a greasy feel, was then treated with dilute spirit of wine. The solution thus obtained, evaporated, yielded a matter of a light, bright yellow hue, soluble in water, and partially deliquescent, consisting chiefly of saline matter and of ozmazome.

Next, pure alcohol was used, and heat employed, nearly to the boiling point. Whilst hot, the solution formed was decanted and evaporated to a small bulk. On cooling, a notable portion of fatty matter was deposited, also of a light yellow hue. The residual fluid decanted and evaporated, yielded only a very little matter of a brown hue, consisting chiefly of fatty matter, mixed, I believe, with a little ozmazome.

The albuminous residue not dissolved either by dilute spirit of wine, or by concentrated alcohol, was in large proportion, compared with the quantity of the substances mentioned, extracted by the spirituous menstrea. The precise proportions of the constituent parts, I have not attempted to verify; a

great deal, I apprehend, remains to be done, before this can be considered of much importance.

The fatty matter, which I have obtained from pus has been similar to that described by the author last quoted ; of a butteraceous consistence ; moderately firm at 60° ; semifluid at 80° ; and liquid at a somewhat higher temperature, according to Dr. Gueterbock at about 140° F., and when pretty strongly heated giving off an ammoniacal smell. Dr. Gueterbock mentions that he could obtain no proof of the presence of cholestrine in it, which recently it has been asserted exists in pus. This substance, he justly remarks, is deposited from hot alcohol in cooling, in the form of crystalline plates : no such plates appeared in any of the processes with alcohol, to which pus was submitted in his experiments ; and I have the same remark to make on mine ; thus indicating the absence of cholestrine. Probably the fatty matter is a mixture of two or more kinds, one capable of combining with alkalies, and forming a soap ; another not, and so resembling and approximating to cholestrine. Dr. Gueterbock appears to be of opinion, that the fatty matter of pus, is free adipose matter mixed with the purulent fluid, inasmuch as adipose globules may be occasionally detected in it with the microscope, and the paper used in filtering pus is rendered slightly translucent.

This conclusion does not appear to me very probable, or well supported ; it seems to me more likely, that the fatty matter belongs essentially to pus,

and enters into the composition of the pus-globule, and that it is to it that the pus owes its colour and its cream-like character. In an instance of phthisis with much emaciation, I have detected it in the pus of a vomica; in which, it could hardly be expected to be found, unless essential.

Of the substances inferred to exist in pus, and not soluble in alcohol, that which has been called pyina is the only one on which I shall offer any remark. Dr. Gueterbock, the discoverer of it in pus, and by whom it has been named, considers its characters well marked as a proximate principle: namely, solubility in water, insolubility in alcohol and acetic acid, precipitation from solution in water by this acid, and non-conversion into gelatine by long-continued boiling. I would not wish to appear unduly sceptical: but I cannot refrain from doubt relative to the positive existence of this substance, independent of the means employed to procure it. It is possible that it may be formed by the action of acetic acid on the albuminous part of the pus. I have stated at page 95 of this volume, that the serum of the blood is not coagulated by strong acetic acid. I have since found, that although there is no effect immediately produced, nor for many hours, yet that there is in the course of many days, and that if the mixture of the serum and acid be kept, after a time it will be found jellied or feebly coagulated; and farther, that the gelatinous compound under the microscope exhibits very minute globules.

Now, is it not possible that this acid on the serous fluid of pus may have a more rapid effect, and produce instantly a substance, a form of albumen, such as that which has been called pyina? The properties of animal and vegetable substances are so easily altered, by feeble agencies, sometimes with, sometimes without, new arrangements of their elements, independent of any alteration in their proportions, that great caution requires to be observed relative to the adoption of new proximate principles.

7.—Dr. Gueterbock has given a very good account of the pus-particles, under microscopical examination, with which my observations perfectly accord. After remarking that they vary in size, and that hitherto those of the greatest magnitude only have been described by authors, he adds,—“*Corpuscula majora formam habent pæne rotundam, et sphæricam, quam, si aqua evaporante magna natant celeritate, luculenter videre potes. Et forma et magnitudine paulo discrepant, alia (et major quidem pars) satis rotunda, alia irregularia, marginibus aliorum acutis, aliorum crenatis, superficie in plerisque rugata, mororum non dissimilis, ita ut, qui primum ea videant, alia granula minora supersedere credat.*”* The

* When pus is mixed largely with water, and thrown on a filter, the greater part of it is retained on the filter; a small portion passes through with the water; under the microscope, the particles of the latter appear more irregular in form, and most of them smaller than those of the former. Also, when largely diluted with water, in a tall jar, and set aside at rest, a difference will be found in the time of subsidence of the particles, some falling down immediately, others

same writer has also described faithfully the appearance of the individual pus-globule, when carefully observed and subjected to the action of different agents. The result of his observations is, that the globule is composed of two parts,—an involucrum, which is rendered more transparent by water, spirit of wine, and acetic acid,—and of central granules, on which the agents mentioned have not the same effect; and, in consequence, they are rendered, by them, more conspicuous.*

Mr. Gulliver, too, has recently directed his attention to the same subject, and, quite independently, has arrived at the same conclusion relative to the compound nature of the pus-globule. This gentleman considers the central granules, or, as he designates them, molecules, as composed of a peculiar principle, possessed of properties similar to those belonging to pyina.†

Of the accuracy of the observations generally, on which this conclusion is founded, I have not the

not for many hours; and these last, on microscopical examination, I have commonly found ill defined in form, and of a looser texture.

* Vide Plate iii. figures 3, 4, 5, copied from Dr. Gueterbock's work, representing, with tolerable accuracy, Fig. 3, recent pus-globules; Fig. 4, the same after immersion in water for some time, by which the transparency of their involucra is increased; and Fig. 5, the central granules: for further illustration a figure (No. 6) is given of the appearance of the pus-globules immersed in acetic acid, magnified about three hundred diameters, seen through the achromatic object glass of one-eighth of an inch focal distance, before often mentioned.

† Vide London Medical Gazette, May 1839, for a notice of Mr. Gulliver's paper, "On the composition and elementary structure of the Pus-globule."

slightest doubt. But I am not quite free from doubt concerning the inference, viz., that the central granules, or molecules, are composed of a distinct substance. Fibrin is not dissolved by acetic acid; the spermatic entozoa are not dissolved by it; coagulated serum is not dissolved by it;* nor is fatty matter; in brief, its solvent power over animal substances generally is very inconsiderable, as I have briefly mentioned, when treating of the action of distilled vinegar.† I have sometimes imagined that the appearance attributed to central molecules might be owing to particles of air, disengaged by the action of the acid on the minute quantity of carbonated alkali, which there is reason to believe exists in the

* Contrary to what Dr. Gueterbock mentions, I have not found the involucrum of the pus-globules dissolved by acetic acid; only rendered more transparent. If, after the action of this acid, an alkali be added to neutralize it, or if a solution of common salt be mixed with it, to change the refractive power of the medium, the involucrum is rendered discernible.

† Doubt, perhaps, similar to that which I have expressed has occurred to Dr. Gueterbock; this I infer, from his remark relative to the composition of pus-globules; that he cannot venture to determine, whether they consist entirely, or only in part of fibrin; and also from the notes to this remark, concerning the action of acetic acid;—"Berzelio duce fibrinam sanguinis bovini et albumen ovi calore coagulatum bene abluta acido acetico concentrato superfudi, ut utrumque intumesceret, at aqua coquente tantum vix utriusque solutum est, ut solutione kali ferroso-hydrocyanici parcum fieret sedimentum. Miratus, quod Berzelio teste in omnibus libris chemicis adnotatum est, et fibrinam et albumen acido acetico solvi, experimentum sæpius repetivi, nonnunquam acido acetico diluto adhibito sed semper idem fuit eventus." He justly adds, "Si hæc experimenta probantur, novos in tenebras et confusiones incidimus, et ad novam materierum illorum simplicium disquisitionem excitamur."

pus-globules. Some facts are not unfavourable to this idea; thus, though the globules are rendered more translucent by a solution of potash, soda, or ammonia, the molecules are less apparent than even when in water, and if either of these alkalies be added to a mixture of pus and acetic acid, the molecules, before very visible, become indistinct. At other times, I have supposed that the appearance in question might be connected with the existence of particles of fat in the globules, on which the acetic acid would have little effect, and might render more conspicuous by increasing the transparency of the enveloping part. The circumstance, that alcohol does not render the molecules more but less distinct, whilst it augments the translucency of the involucra, is rather favourable to this idea. I venture to express the foregoing doubts, and to offer these conjectures on this obscure subject, with hesitation, and the hope, that they may lead to further inquiry. In justice to Mr. Gulliver, I should remark, that I have not read his paper referred to on the composition of the pus-globule; the volume of the Transactions of the Medico-Chirurgical Society, in which it will appear, not having yet been published.

8. Relative to that important question in pathology, the mode of formation of pus, it is matter of very interesting inquiry, whether it is a secretion depending on a peculiar state of vessels in the part in which the fluid is collected, or whether it may not be formed in the general mass of the circulating

blood, and be poured out merely where it is found collected. The phenomena of ulcers are favourable to the first view; and seem at least to prove, that in certain cases, its origin is local, and that it is formed in the part where it presents itself. On the other hand, the collections of pus which so often occur in erysipelas, in deeply seated parts, remote from the cutaneous inflammation, and without symptoms of corresponding local inflammation, favour the latter view. This view is also favoured by the detection of globules resembling pus-globules in the blood, during the process of suppuration,—whether in instances of superficial suppuration, as in small pox, or in deeply seated, as in tubercular phthisis, or lumbar abscess; and the analogy which there is in a certain degree between the puriloid matter, occasionally found in masses of fibrin, in the cavities of the heart and of the great vessels, seems to me likewise to support it. Inquiry now is happily directed to pyogenesis, and with the aids which the collateral sciences are capable of affording, there is good ground for hope, that its termination will be successful; and if successful, that it may be the commencement of a new era in pathology.

THE END.



Fig. 1.

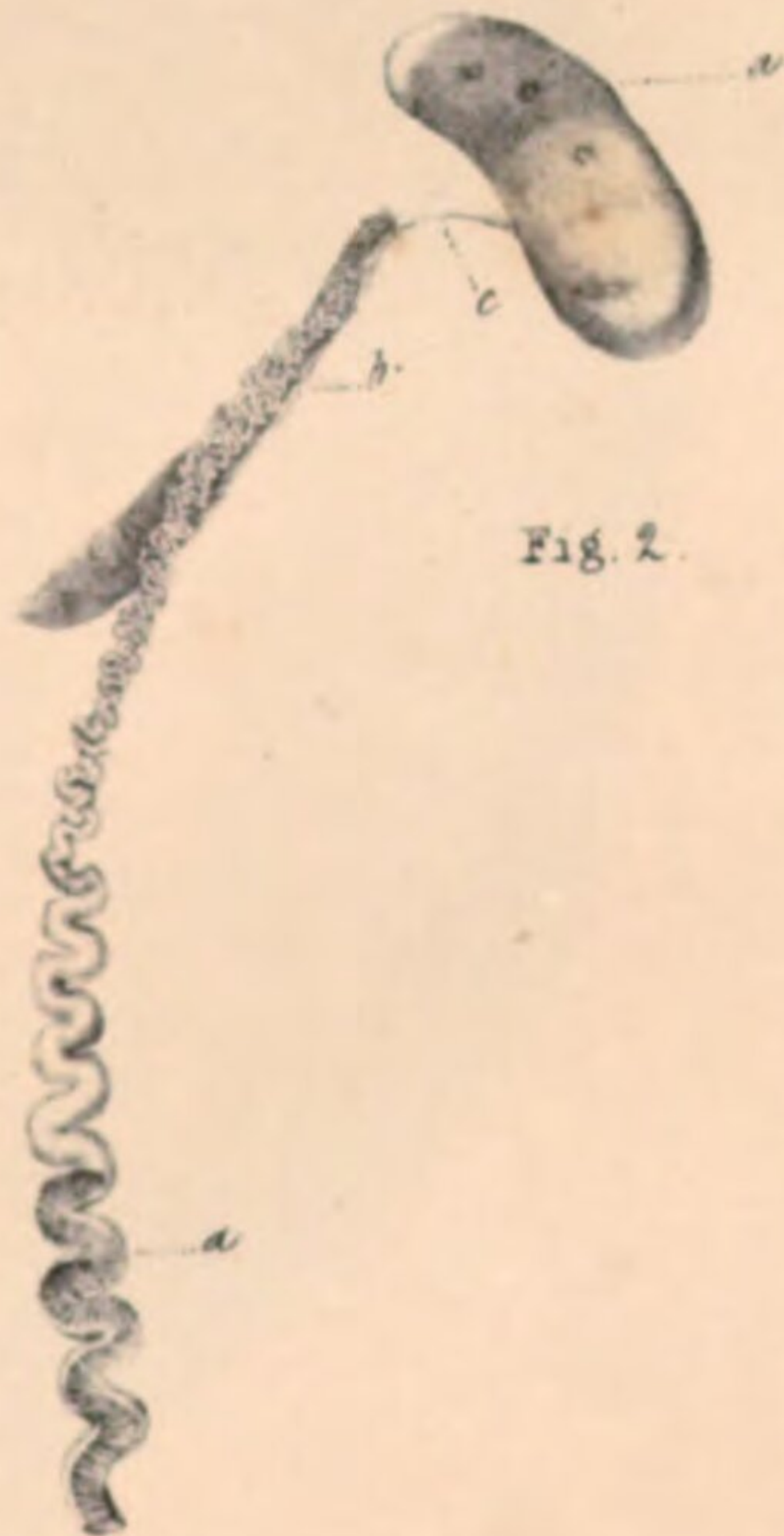


Fig. 2.

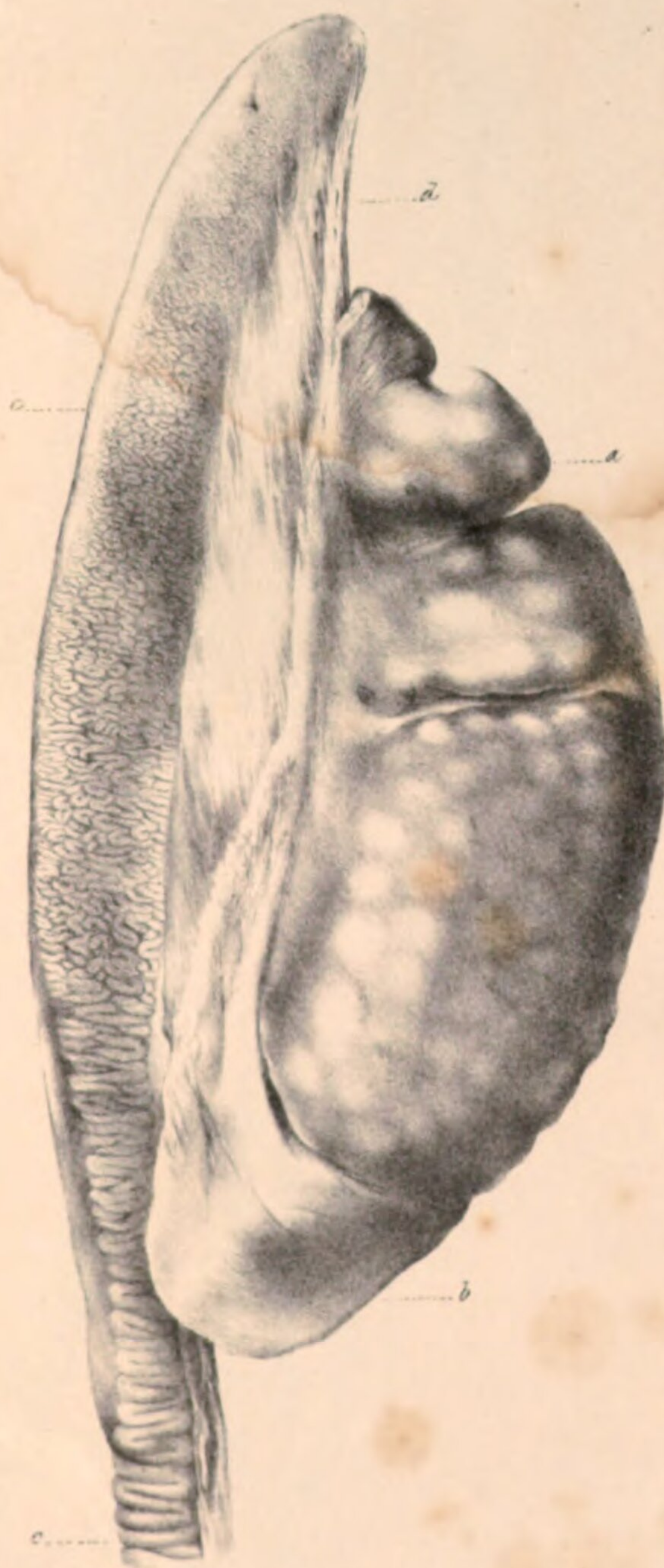


Fig. 1.



Fig. 3.



Fig. 4.



Fig. 5.



Fig. 6.



Fig. 2.



Davy, John

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